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MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

EDITED BY

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A QUARTERLY REVIEW

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I.—COMPETING CRITERIA

BY F. S. McNEILLY

So far as we accept some rules and apply some criteria, we reject others. In the formation of our moral beliefs we are consumers faced with a choice of competing products. My concern is in part to investigate the conditions that obtain in this market, and in part to make some suggestions about how a discriminating buyer should conduct himself. I shall be specially occupied in discussing the part that may be ascribed to the expression of attitudes in the analysis of moral judgment, since I think that this has recently been much misrepresented. There are some philosophers today who are selling excellent wares, but in the wrong market. I begin, then, by discussing the connexion between attitudes and the expression of moral opinions ; I argue, secondly, that the matter of sincerity of assent to moral principles is both relevant and more complex than has been supposed ; and suggest, finally, an account of moral discourse which, while familiar, has the merit at least of being, to the best of my knowledge, one of the few attempts in years to get away with committing the Naturalistic Fallacy.

1.   *The attitudes of others*

According to Mr. P. H. Nowell-Smith, the unqualified use of the word " right " implies a background of general agreement :

> Smith asks Jones for advice because he believes Jones to be
> wise and experienced in the ways of the world ; and if Jones
> gives unequivocal advice, Smith is entitled to infer not only
> that Jones himself approves of the lines of conduct he calls
> ' right ', but also that it is generally approved. (*Ethics*, p. 189.)

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If there is no general agreement, or if Jones knows that his views are unusual, he ought to add something like " that is my personal view ; but many good people think otherwise."

In one respect this contention is undeniable. Certainly there is occasion for modesty. Unless one has good reason for claiming authority superior to that of the majority, it is a gracious thing to make the position plain to one's audience so that they shall not possibly be misled. But this applies to the expression of any opinion whatever and is not a special feature of moral discourse. If someone were quite sure that the rest of the world was out of step and said without qualification that something was wrong, knowing that almost everyone else thought the contrary, we might accuse him of error, or of rashness, immodesty and intolerance ; but not always of dishonesty, and never of an inability to express himself correctly. By all means let us urge that it is not the thing to say unequivocally that the world is flat when our audience is unacquainted with the verdict of informed opinion. This is a requirement of the decent but not of the " correct " use of language ; and it has no special place in the analysis of moral uses specifically. ·

Further, Nowell-Smith holds that " you ought " sentences

> are addressed to a rational agent as solutions to his problem of choice and, in consequence, they imply a pro-attitude on the part of the recipient (p. 192).

While this seems well to distinguish moral judgments from the issuing of commands, which is what Nowell-Smith intended, once again it does not serve as a distinguishing feature of the moral use of words generally. To begin with, Nowell-Smith is being a little too pessimistic. Supposing—what I do not believe to be the case—that the function of moral judgments of the sort in question is to assist in the making of a decision, a lack of the appropriate pro-attitude in the hearer need not muzzle the speaker. In many circumstances moral discourse can have, although not as an essential part of its functions, some effect of inducing dispositions. Parents, for example, may use moral language to inculcate moral attitudes and dispositions, and not merely by way of appeal to ones already established. You might say, of course, that their children must have, and that they do indeed have, a pro-attitude towards adopting the pro-attitudes of their parents. But it will need to be shown that this is anything more than a disposition to accept parental authority generally. Further, the fact that one must be able to appeal to such a disposition does not serve to mark off moral from any other

sort of discourse. If people did not have some small, although not an overwhelming, disposition to accept the word of others, communication of all sorts would be more difficult than it is.

Moreover, if I express my moral beliefs in the categorical sort of way that demands assent, it is not necessarily because I think it is there for the asking, but perhaps because I think that agreement is the reasonable response. *Prima-facie* one would say that what was appropriate to " You ought " was agreement or disagreement—" Yes " or " No, there's no reason why I should " ; and not, except at the nursery level, submission or contumacy—" I shall " or " I shall not " ; nor yet a charge of irrelevance—" Ah, but *I* happen to have a con-attitude ! " The question may at least remain open whether there are not other reasons for hoping for agreement than the ready-made pro-attitudes of the audience.

2. *The attitudes of the speaker*

That an utterance should express attitudes of the speaker is neither a necessary nor a sufficient condition of its expressing a moral judgment, no matter how complex and consistent these attitudes may be, and no matter with what degree of generality any rules involved may be formulated.

I might say to someone, " I advise you to do x ; x is in accordance with Rule 1, to which I hereby subscribe, and which is formulated universally ; Rule 1 rests on Rule 2, which is formulated universally, and to which I hereby subscribe ; Rule 2 rests on a pro-attitude ; I have this attitude ; so have you ; so has everyone." If Hare and Nowell-Smith were right I would then have said, perhaps a little more, but scarcely any less, than I can ever say in " You ought to do x ". The chief difference, I suppose, would be that in saying " You ought " I would only " imply " the rules and attitudes in question.

The trouble is that the term " attitude " is notoriously unsatisfactory. Nowell-Smith uses it as a generic term " to cover desiring, liking, wanting, approving, enjoying, being fond of, interested in, and so on " (p. 111). But this list only makes it the more obvious that it is not any essential function of moral language to express attitudes. The use of " right " Nowell-Smith alleges, implies a pro-attitude (p. 186). In the first place there are some at least who would scrutinise this list and firmly deny that when they used " right " in a moral context they were expressing any of these attitudes, except perhaps approval. Then again, approval is a word which is not without its

difficulties. In *The Language of Morals* Hare tends to suggest
that the typical use of " approve " is performatory. The minister
of local government might have a letter written to me saying
that he approved of my plan, and if he did it would be silly to
try to confirm the letter by having someone observe him for
signs of emotion. In these circumstances to send the letter is
to approve. (*The Language of Morals*, p. 10.) This is all very
well. But there is " approve " and there is " approve *of* ".
To approve is to confirm or sanction, and to approve of some-
thing is to consider or pronounce it good. If I have the appro-
priate authority, then in approving of a thing I may be sanction-
ing or approving it. But I can approve of a thing without being
in a position to sanction it, and I can sanction it without approv-
ing of it, although not without approving it. If " right " is
tied to approval it is to approval *of*. I can appraise the be-
haviour of anyone, if I choose, but there are few whose conduct
I am entitled either to sanction or to prohibit.

But secondly, the question is whether " approval " is a pro-
attitude at all, in the sense which Nowell-Smith requires. In
elucidation of his use of the term " attitude " he cites Hobbes's
distinction between " endeavour toward " and " endeavour
fromward " : his pro-attitudes correspond to the former and his
con-attitudes to the latter. That is to say that, as one would
expect from the list of attitudes which he provides, an attitude
is or involves a disposition to make certain sorts of choice or
decision. This comes out very obviously in his account of the
logic of the expression " I thought it right ". This expression,
he says, implies a pro-attitude of the speaker and cannot be
separated from the decision to act ; so that it would be queer to
say, " I know you thought it right, but why did you do it ? "
(p. 186). But Nowell-Smith is claiming too much. Whether or
not such a question was queer would depend on the context in
which it was asked. It would be queer if I had already asked
why he did it and were seeming to ignore his answer. On the
other hand, I could be asking for more information : " Did you
do it, thinking it was right, out of a sort of blind dutifulness ; or
out of a concern for others ; or out of " self-respect " ; or
what ? " Again, in giving his thinking it right as a reason
for what he did a man of course implies a pro-attitude. But this
is not because the standard use of " right " implies such an
attitude, but because *any* answer to the question " Why did you
do it ? " implies a pro-attitude : otherwise it would not be an
answer to the question. It is only in such special circumstances
that the use of " right " implies a pro-attitude, and then what

implies it is not the use of " right " specifically but the mere answering of the question. So far, then, as the use of " right " implies an attitude of approval, it is not the sort of attitude that is " tied to the decision to act ".

The same is true of " ought " also. Nowell-Smith's account of the latter is more complex in some respects, but it is vitiated by the same error. He distinguishes two different senses which " I ought " may have. (1) To " express a verdict or decision ". Used in this sense it would be logically odd to say " I know I ought to do x but shall I do it ? " But (2) it is used also not to express a decision but in the course of deliberation. In this second sense it is as if some " internal moral authority " or " the voice of conscience " spoke—and, at that, said really not " I ought " but " You ought ". These are, Nowell-Smith says, the voices not of " judges " but of " advocates ". They speak either for competing moral principles or as the voice of conscience against some other motive (pp. 261-262).

Now in the first place it is exceedingly queer to use " verdict " and " decision " in the same breath. Far from agreeing that " I ought " may " express a verdict or decision " I should suggest that so far as it may express the one it may not express the other. The metaphor of the judges and advocates ought to have been pushed further. There is quite a close parallel to be found, for what it is worth. A jury may deliberate, and I may deliberate. The jury may reach a verdict of " Guilty ", as I may reach the verdict that I ought to tell a lie on this occasion. Sentence follows legally on the verdict, in the sense that if it is not passed, except for some legally good reason, the procedure of the court is so far unlawful. My decision follows morally on my verdict in the sense that if I do not decide, except for some morally good reason, my own procedure has been immoral. But neither follows logically upon the other. The " follow " is in the one case in virtue of the laws of the land and in the other in virtue of moral principles. Nor does hanging follow logically, but legally, upon sentence of death. Nor does telling a lie on this occasion follow logically, but morally, upon the decision to tell the lie. It would not, of course, be right to implement the decision if there had been a miscarriage at some point in my deliberations. No more would it be proper to carry out the sentence if there were known to have been a miscarriage at the trial. Sentences, like decisions, depend on verdicts, but are not to be identified with them ; and their dependence is not according to the laws of logic or of language, but according to the laws of the land or of morals.

Secondly, the only use of " ought " which is tied to decisions is the non-moral use. Certainly if I can say that I ought to use a match and not a lighter I have *ipso facto* a good reason or motive for using a match : for I would not agree that I ought if it were not that I did not like the taste of petrol, and had no equally strong preference on other grounds for the use of the lighter. But it is not obviously true that the same applies to the moral use. It does not, in fact, even apply to the non-moral use in the way in which Nowell-Smith supposes. His account of failing to do what one knows one ought is that I can say " I ought to do x, but I shall not " only if I am using " shall " predictively and not to express a decision (p. 268). That might be, and " ought " still not be tied to a decision. What would be nearer the truth would be to say that to come to the position of being able to say " I ought " is to come to the position of being able to make a reasonable decision without further reflection. One may, however, be unreasonable in the event : one can know that one ought and through weakness fail to decide.

I do not think, then, that any convincing case has been made for alleging that attitudes, in the sense of Hobbesian " endeavours ", are implied in specifically moral uses of " right " and " ought ". But it might be, of course, that a less practical sort of attitude was involved ; and indeed it not only might be but certainly is. If I think that mercy-killing is right I have *ipso facto* a certain attitude towards mercy-killing. But in this sense of " attitude ", Hare, for example, has seen that we are liable simply to go round in circles : to explain the use of " right " as the expression of an attitude and then to have to explain the attitude as the disposition to think that this or that is right (p. 11). Perhaps there are some attitudes which are moral and some which are not. So one would think ; and if one is correct in so thinking a discussion of attitudes gives us small help. At all events I do not deny that moral judgments express attitudes but only that they express anything remotely similar to Hobbesian endeavours.

Although it is not of the essence of the moral use of language directly to express practical attitudes, however, it may still be that these are involved in moral judgments in a different way. For it is very commonly held that really to assent to moral principles is to be at least to some degree committed to act in accordance with them. This has been used as a stick to beat the Intuitionist with, since it is not obvious what there can be about the scrutiny of any object, no matter how utterly non-natural it may be, that can necessarily involve commitment to

certain sorts of behaviour. My complaint is not that the Intuitionist ought not to be well beaten, but that this stick will not hurt.

What matters is what the conditions are under which a person may be said to assent sincerely to some moral principle or judgment. Hare found a problem here, which he admitted without attempting to push a solution very far (pp. 169-170). Mr. Gardiner takes Hare to task for tying sincerity too closely to performance and suggests that failure to perform does no more than raise the question of a man's sincerity, without answering it. But Gardiner thinks that failure to perform does make a presumption of insincerity, for his argument is that while one who consistently fails to act upon the principles which he claims to accept may not be sincere, he is not necessarily wholly insincere. Lying about what one has done and why one has done it may, Gardiner suggests, be allowed to count against total insincerity. Disposition to act upon a principle he regards as a criterion, and an important one, of sincerity of assent, although it is not the only one. (P. L. Gardiner : " On assenting to a moral principle ", A.S.P., 1954-55.) I think, however, that this also is a mistake, and that a disposition to act conformably to a principle is not so much as one among several criteria of sincerity of assent, but merely a fairly reliable sign. In order to be in a position to argue my point, however, I have to digress briefly.

3. " Implications " of statements

The useful distinction is now familiar, between what is entailed by a statement and what is " implied " in the circumstances in which it is made. Nowell-Smith uses the terms " logically implied " and " contextually implied " respectively. Jones's saying " It is raining " contextually implies that Jones believes that it is raining, while an unqualified use of " right " implies agreement with the general consensus of opinion (pp. 81, 189).

I need this notion of contextual implication for my own purposes, but in a slightly different form. I propose to use the expression " concomitants of an utterance " to designate any moods, feelings, attitudes, dispositions, beliefs, " states of mind " of any sort of the speaker at the time of the utterance. It is an ugly expression, but I can think of no better. Now there are some concomitants which do in fact more often than not accompany certain sorts of utterance. The belief that the cat is asleep is a usual concomitant of the statement that the cat

is asleep, except in ironic or quoting uses. But the belief is also
an " implied " concomitant. What I mean here by " implied "
is precisely this. There might be a possible concomitant of a
statement such that the hearer's knowledge that it was absent
would deprive him of any grounds, other than those ascertained
independently of the speaker's assurance, for accepting the
statement. Such a concomitant would be " implied " in my
sense. Thus the speaker's belief that the cat is asleep is implied
by his statement that it is asleep ; for if there were no evidence
available about the state of the animal and the hearer knew
that the speaker did not believe that it was asleep, he could
have no grounds for accepting the speaker's statement that it
was.

But a concomitant may be " suggested " without being
implied. I might simulate a snobbery of which in fact I was not
guilty by discoursing at length on the social standing of my
friends. Snobbery is a common motive for discussions of that
sort, and if a usual occasion is in no way disavowed the sugges-
tion, even if only by default, is that it is present. But a usual
or suggested concomitant need not be implied. Malice might
be a usual concomitant of the telling of discreditable stories
about one's friends : but your knowledge on some occasion
that I was *not* being malicious could hardly prejudice your
acceptance of my slanders, but might incline you to heed them
for once.

4. *Sincerity of assent*

The sort of view which I am anxious to attack is that which
identifies assent to moral principles with some sort of disposition,
however weak or strong, to act conformably to them. For the
sake of brevity I shall call the disposition to act in accordance
with some moral judgment *commitment* to that judgment.
Even so much as to feel compunction in not acting comformably
would constitute some small degree of commitment, since, in the
absence of any contrary motive, a feeling of compunction would
be a spur to act conformably. Now my suggestion will be that
believing in a moral principle, or honestly assenting to it, is not
essentially a matter of commitment at all. If it were, then
commitment would be an implied concomitant of moral
appraisal ; and I think that that can be made to seem more
doubtful than it may appear at first sight.

There is a preliminary point to be settled here about the word
" sincere ". There is a sense of the word in which it would be
correct to talk of someone of an open and strongly amiable dis-

position as a sincere person, when it would be odd or unusual to describe a person of openly malicious temper in the same way. Then again the term is often used, if not exclusively in relation to an amiable disposition, at least in connexion quite specifically with the expression of feelings and attitudes. But all that I am concerned with is the minimum sense of the term, allowed by the dictionary, in which it implies merely an absence of deceit. What I am concerned with are the conditions under which someone may be said honestly to believe in a principle. He sincerely assents to it so long as, understanding the use of the words he employs, he is not dissembling.

Now in making a moral judgment one might apply some criterion : let us say, considerations of social utility. Then there are many possibilities. Someone might say that it was not wrong to tell lies, when in fact he had given the matter no thought at all, had applied no criteria, but simply wanted to give an impression of unorthodoxy. Again, someone might have thought about the matter and come to a conclusion contrary to the one expressed, saying that it was not wrong, but actually thinking that it was. Or someone might have thought about the thing, coming to the conclusion that it was not wrong, and expressing it honestly. Already it is possible to talk about sincerity and insincerity. In the first two cases the speaker is insincere : in the first because he expresses an opinion when he in fact has no opinion on the matter at all ; and in the second because he has a contrary opinion to the one he expresses. In the third case there are at least grounds for calling the speaker sincere. Of course it may be claimed that attitudes and dispositions have to come into the matter sooner or later : that in the third case the man is not sincere if he has no degree of commitment. No doubt there is something to be said for this view, and it is often enough taken for granted. But it ought not to be swallowed whole, even if it cannot be altogether denied. What matters, at all events, is the precise respect in which it is true.

We need to look closer at the ways in which it is possible to be misled by insincere affirmations. In the first place, we are liable to be misled about the speaker's beliefs. If he says what he does not believe, then obviously we may be misled in this way. Secondly, whether or not he believes what he says, the speaker may mislead us about his aims, attitudes, interests, and tastes. A person might discourse on the plight of the underprivileged, not in order to gain assent to the views he expressed, but in order to give an impression of being concerned about the

welfare of his fellows. Thirdly, we may be misled concerning the topic of the statement in question. Clearly if Smith says that Jones is in London, in fact not believing that he is, his hearers are liable to be misled about Jones's whereabouts. Now the view which I am criticising would allow no such three-fold division of ways of being misled by insincerity in the expression of moral beliefs. Not really to believe in a moral principle would be simply not to have the appropriate attitudes, and there would be no difference between being misled about a person's moral beliefs and being misled about his attitudes. Or if we are abandoning the narrower sort of attitude theories of moral language and are concerned with the view that commitment is at least one of the things implied by assent to a moral appraisal, then still it would follow that one cannot be misled in the first sort of way without at the same time being misled in the second.

But that is not the case. The variety of ways of being misled by insincerity is the same where the beliefs are moral as where they are not. This becomes evident if one applies the distinction between implied and usual concomitants. A person might discourse at length on the technique of the fugue, on the composition of this or that item of the Forty-Eight, on the structure of the late Beethoven quartets, and so on. If he did, and if there were no indication of extraneous interests—mathematical, or psycho-analytical, or whatever it might be—then the assumption would be that he had a taste for contrapuntal music. If the speaker in fact found such music dismally boring he could be accused in such circumstances of misleading his audience. We are all familiar with techniques of simulating tastes, interests and attitudes. In the circumstances specified a taste for contrapuntal music is *suggested :* the audience's belief that the speaker has such a taste is something contrived by his discourse ; if it was not intended, he has been inept. But although it is suggested it is not implied. The knowledge that a person was simulating an interest would not of itself deprive us of grounds for accepting the statements on authority. It would depend entirely on the situation. In many cases an interest could be effectively simulated only by means of knowledgeable and informed conversation ; and in every situation of the sort which we are considering the end could be more surely contrived by the expression of informed beliefs than by an exhibition of ignorance and error. In these cases it is possible to distinguish between suspecting that a person does not believe what he says and suspecting that his reasons for saying it are dishonest although it is not doubted

that he believes it. If one thought that the speaker had performed certain procedures—of listening to musical performances, of examining scores, and so on—and had come to certain conclusions ; or if one thought that he had done no more than to accept the authority of those who had performed these procedures and come to these conclusions ; then one could reasonably think that he honestly believed what he said although his saying it was dishonest in another respect.

The same is true of the expression of moral beliefs. At the very least there are evident some very close analogies. It would be one thing to know that the speaker had given the matter no thought at all, or that he had come to a conclusion different from the one expressed. To the extent to which one knew this one would necessarily be deprived of grounds for accepting what he said. But there are circumstances in which we could know that the speaker had no commitment and yet have to give serious consideration to what he said. If we thought that someone was applying criteria to a complex case, viewing the facts and making a balance of interests with an insight, which we ourselves lacked, into the strivings and aspirations of the people involved, then even if we knew that he was not committed, so long as we had no reason to suspect dishonesty or negligence, we would have some reason for assenting. The fact is that there would be more good reasons to accept the appraisals of one who, although uncommitted, had a sensitive understanding of people generally, then there would be to accept those of a well-intentioned social misfit. That is to say, there would be more good reasons so long as one had grounds for supposing that the speaker was not actually being dishonest. That is a big qualification. It might be difficult to find anyone who was totally uncommitted. If one did, the probability is that he would be totally without occasion ever to make, let alone honestly to express, a disinterested moral appraisal. This, however, would be, humanly speaking, rare ; and not, logically speaking, impossible.

But of course this all depends on a point which I have not yet even tried to establish. The theories from which I have been dissenting have at least one defence which they could offer. It could be said, as it has been by Hare, that a purely descriptive use of moral language is possible. We might say that such and such a thing was good, although we ourselves would not commend it, but indicating that people normally were agreed in commending it. We might not accept other people's criteria and yet use commending words to indicate that a thing satisfied

the criteria which those others accepted. This would allow us, then, reasonably to accept someone's appraisal when we knew him to be uncommitted, so long as he was using words in this merely descriptive sort of way and was applying criteria which we would accept. But this would be to say, Hare would argue, that the very case in which commitment was not an " implied concomitant " was that in which moral language was being used not commendingly but descriptively ; and this, although it is a perfectly proper use of such language, is not the moral use, since the latter is primarily commending. Hare's argument here is that appraisals could not have a descriptive force at all if they did not have a primary commending use.

The argument would be more convincing if the notion of commending were more transparent. In this matter dictionaries are less helpful than entertaining. The philosopher makes the first move : to make a moral appraisal is to commend. The dictionary takes over : to commend is to praise, to praise is to express warm approbation of ; approbation is approval ; and to approve of something is to consider or pronounce it good. *Of course* to say that something is good is to commend it : but the whole question at issue is whether or not it is silly to say that commending is a sort of describing. I am not in any position to argue that it is not silly, since I am concerned with moral appraisals only, while most of the commending that gets done in the world is not a matter of morals at all. If the account which I shall be suggesting is correct then moral appraisals will be better described as logically complex sorts of descriptions than categorically distinguished from them. I think that the chief difficulty which I shall have to overcome is the now general and obsessive fear of committing the Naturalistic Fallacy. At least I shall succeed in committing it myself without blushing ; and I shall try to defend my immodesty.

For the issue is becoming plain. All that I have written is nonsense if anything like the account which Hare gives is true of moral discourse. If to know that someone was expressing a moral appraisal was simply to know that he was trying to invoke some or other criteria, issuing in injunctions having a very general application, then unless one knew what these criteria were and accepted them, one would be in no position reasonably to take the man's word for his " conclusions ". He might be committed to one sort of life while one might oneself be committed to a quite different sort. On the other hand, if there were only one criterion which could sensibly be represented as a moral one, and if we had reason to believe that the man was not

a fool and was trying to make a genuine moral appraisal, then what I have said would be not nonsense but evidently true. The matter is certainly not going to turn out to be so simple as that, but for the moment it will be enough to toy with the idea in that very crude form.

We may consider Hare's account of the " Fallacy ", which is this. If someone defines some value-word, V, in terms of some conjunction of predicates, C, and then attempts to deduce a moral judgment from a set of descriptive predicates, we can ask whether he would want to commend anything for being C. " If he says that he does, we have only to point out to him that his definition makes this impossible, for the reasons given. And clearly he cannot say that he never wishes to commend anything for being C ; for to commend things for being C is the whole object of his theory " (p. 93). But this argument will not do. We could insist that only one certain criterion was to count as a moral criterion ; and we could say that only something having the character C could satisfy this criterion. We would then be committed to the proposition that to say that anything was, morally speaking, good was to say that it had the character C. But that would not mean that we could not commend a thing for being C ; it would mean at most only that we could not commend a thing *morally* for being C. Not all commending, however, is moral or intended as such. If we had to deal with a thoroughly delinquent person who exhibited no concern at all for what we called moral considerations and made no claim to any " morality " of his own, we *might* have him admit that this or that was good while announcing that he intended to do nothing at all about it. He might, after all, take a pride in his shamelessness, and have enough self-respect to claim wickedness and not ignorance or " maladjustment ". We might well commend a certain sort of life to him, and try to induce him to accept responsibilities and to take concern for others. If we had enough time and were wise enough in dealing with such people we might even have some success. These would be important commendations that we were making, but they would not be moral commendations. They could not be, for *ex hypothesi* appeal to moral considerations is useless and will gain only theoretical assent at best. Yet we could still commend some way of life, in the hope of bringing the hearer to a state in which there would be some point in having a moral discussion. That is to say that there could be situations in which the morally important thing to do was to commend, although not morally to commend.

The situation, of course, is less simple than that : we cannot

say that there is any single criterion which alone can be properly
described as a moral criterion. If the Naturalistic Fallacy is to
be committed, it must not be too crudely. In order to arrive
at the best way of committing it it is necessary to turn to the
question of what it is for a person's assent to a rule to count as
assent to a *moral* rule.

Before considering this, however, there is one final point which
tends to confirm my arguments about sincerity of assent and
will help to prepare the way for the remainder of what I have to
say. It is that whether or not it is true that there can be sin-
cerity of assent without any commitment, at all events *degree*
of assent does not vary concomitantly with degree of commit-
ment. One might assent tentatively and be fully committed,
or assent with full conviction and be weakly committed. Weak
commitment to the moral rules to which one assents, whether
tentatively or with conviction, is a matter of character : either
of weakness of character, or of strength in attachment to immoral
policies ; while tentative assent to moral rules is a matter of
modest or hesitant judgment.

5. *Assent to moral rules*

It would be no use enquiring what sort of rules could be held
to be moral rules, for the only answer would be that any sort at
all can, there being no known limit to human stupidity. But it
would be quite another matter to ask what sort of reason for
assent to a rule would constitute it assent to a moral rule. Pro-
visionally I suggest that we should say that a person assents to a
rule as a moral rule only if he assents because he recognises that
the rule at least partly satisfies the following two criteria, which
are obvious and familiar. (1) *Equity.* A moral rule cannot
favour the interests of any particular people as against any
others. (2) *Utility.* A moral rule must be such that its general
observance over a period as long as it is possible to calculate
will advance the interests of people generally more than it will
impede these. That is putting it very baldly. Of course I must
qualify. The remaining few pages suggest briefly how that
claim might be effectively defended. I have six chief points.

(1) The requirement is clearly too rigorous as it stands. To
begin with I can turn it upside down and contend that if anyone,
aware that a rule was *wholly* inequitable and such that it would
be in *no-one's* interest to have it observed, were yet to accept it,
we could hardly describe him as assenting to it as a moral rule.
Certainly we could say that someone accepted a rule as a moral

rule even if we saw it to be patently inequitable and of no utility at all. But I think that the correct thing to do with someone who appealed to such a rule would be to press him further and to let his answers decide the issue. If it did not *apply* to him he could not be said to accept it as a rule : he would be suggesting a " morality " for others, perhaps, but not accepting it himself. Then he has to face the possibility of the rule, if acted upon, inhibiting the pursuit of his own interests. There are two possibilities. The one is that his own observance of it might clash with his interests. He could say, if he chose, that he would not then observe it ; and if he still assented to it he would be in the position of the delinquent, which we have discussed. The other possibility is that the observance of the rule by other people might clash with his interests. If he were to accept the rule only on the condition that it could not apply in such a way, then he would not after all be accepting it as a moral rule. If, however, he regarded it, however mistakenly, as having some utility, then there would be a case for saying that he accepted it as a moral rule. Now if he were to say that it would apply so far as it had utility for his own society—or, what is the same thing, were to regard it as having utility but were to conceive of its application only within his own society—then he could be asked for an account of why, when he was willing to allow consideration of the interests of other people, he should draw the line at that point. It could be, of course, that he identified his interests with those of his own society : but so far as he accepted that as a reason for assenting to the rule, so far he would not be accepting it as a moral rule. You might say that he was accepting a sort of moral rule, but not accepting it *as* a moral rule. He might take his stand at some point and admit the relevance of the interests of some other people, but only of some. Then you could say that he was accepting it as a moral rule, but in a largely unreasonable sort of way : making arbitrary exceptions to a rule which he claimed to accept—if not arbitrary, then interested, and not moral. You could have the familiar sort of series : " family " morality . . . " tribe " morality . . . " universal " morality. These, however, are not equal competitors for our favour ; on the contrary, they are such that it would not be reasonable to accept a morality lower in the scale and refuse a " universal " morality, except on grounds that would lay one open to the charge of not really accepting it *as* a morality.

(2) It could be objected that people might accept rules quite unreflectively, and that it would be narrow-minded to claim that then they were never accepted as moral rules. On the other

hand, so long as one could say that at least in effect the rules were regarded as having some degree of equity and utility, then one could not say that they were accepted *wholly* unreflectively. Still, if the people showed an equal disposition to accept rules which they had been persuaded were inequitable and lacking in all utility, it would be so queer that one would not know what to say, except perhaps that they were lunatics. There are always, of course, those who accept rules because they believe that they have been laid down by God. Now if a man regarded God primarily as a God who loved his world, one might say, conformably with my argument, that he accepted God's rules as moral rules ; and if he regarded God primarily as a punisher of transgressors, one might say that he accepted them on prudential grounds ; but if he regarded God in neither light and still accepted the rules, then one might say that he was a fool.

(3) Again, it might be objected that not all philosophers have been " Utilitarians " : that I am taking too much for granted. But in the first place, a sufficiently sophisticated " Utilitarianism" can avoid at least many of the anomalies. One of the stock examples is the sacrifice of an innocent party to the interests of society. This is perhaps fatal to any attempt to make the appraisal of an action depend exclusively on a calculation of its effects. The argument is that one can exclude from consideration Mill's sort of arguments about the damaging effects of the knowledge that such an injustice had been committed by postulating that society is unaware. But what I am concerned with is the utility of *rules*, and it is absurd to suggest a rule, universally accepted, to the effect that under some circumstances people may be sacrificed without the raising of any apprehensions in society at large : mere acceptance of such a rule would be sufficient grounds for apprehensions. Secondly, great elasticity is possible. It is one thing to accept it as a rule that the interests of others are to be considered : that is something very like a " formal " requirement, in Kant's sense. But it is quite another thing to determine what these interests are. It has not only to be discovered what people take their interests to be but it has also to be investigated—cautiously, no doubt, and with modesty—what these interests really are. Material security, the uninhibited pursuit of one's ends and ambitions, the freedom to work out one's own salvation : these things and many others have to be balanced against each other ; and we all have to do that, at least on our own accounts. If we thought that a person's chief interest was his free choice of his own salvation or damnation, our moral conclusions would be vastly different from those

of some " Utilitarians " of the well-meaning and socially active sort.

There is room here for such a vast amount of disagreement that it might seem that there was no saying what was the morally right thing to do. Although there is room for disagreement, however, it is also possible to specify procedures for removing it. It is not a question of what sort of life is good in the sense of being noble and edifying ; it is primarily a question of what sort of life I can value, and you can value, and so on. Here Mill's test is still open to us. It is something quasi-empirical : look and see. I can reasonably regard one sort of life as better, for me than another if, having an informed choice, it is the sort which I would in fact prefer. No doubt a *fully*-informed choice is not possible. But it is a matter in which, at worst, one may talk of well or badly informed guesses and of at least partial calculations ; and not of, for example, a merely hopeless collision of attitudes. At this point, indeed, the practical attitudes which I sought to dispense with in the analysis of moral discourse have again intruded, and at my own invitation. But there is no inconsistency. I do not deny that my " attitudes " must determine at least in part what I regard as being in the general interest. What I deny is that it is impossible to make a calculation about the latter without having a specific pro-attitude towards *pursuing* it.

I am aware that two paragraphs are not enough to show that philosophers who object to " Utilitarianism " ought to agree with my suggestions. I have done little more than hint that there is some reasonable hope that the differences might be substantially accommodated. At all events I can claim to have made a *hypothetical* case, resting on an hypothesis that is not completely absurd.

(4) I should like to return briefly to the matter of sincerity of assent. A more or less active concern with the interests of at least some other people is perhaps the almost invariable, as it is a specially appropriate, occasion for the making of a moral judgment. But at least rarely another sort of occasion is possible. One might conceivably have interested motives for wanting to see someone else come to the right conclusions in moral matters ; and one might even, in a Satanic sort of way, want to discover what was really the best thing to do so as to accomplish the greatest degree of villainy in neglecting to do it. You could not decide to do something for its wickedness unless you had honestly come to the conclusion that it was the wrong thing to do. It is still true that in nearly all cases it would be

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correct to regard as insincere someone who expressed moral appraisals without intending in any way to act upon them. But this is because it is seldom that anyone has purely selfish or Satanic sorts of occasion. The usual sort of occasion is some degree of concern with the interests of other people. It is so usual that it would normally be misleading to talk morally without avowing one's unconcern. People are justified in treating moral discourse as a sign of some degree of willingness to act morally. But it is still only a matter of signs and of evidence, and not of criteria.

(5) A last word on the Naturalistic Fallacy. I have not suggested identifying the meaning of any " value-word " with some single and unambiguous criterion. I have suggested that the *moral* use of value-words, or of any other words, is to indicate the application of some criterion belonging to a determinate series of possible criteria. It is still open to me without tautology to say, for example, that something ought to be done because it is in accordance with a thoroughly equitable rule having maximum utility. That would not be tautologous. For the most, on my account, that someone could strictly infer from my saying that a thing ought morally to be done would be that I was appealing to a rule, determined by some criterion such that consistency would compel me, if I had the wit and the desire to be consistent, to apply the criteria of total equity and maximum utility. Explicit mention of the criteria would among other things convey information about just how consistent I was succeeding in being. There may be many good reasons for objecting to this way of committing the Naturalistic Fallacy : but the argument about tautology does not touch it.

6. *Conclusions*

If there is a moral dispute then there are in the first place the " empirical facts " to be determined. Secondly, there is the matter of what are the interests of people generally. The calculations here, I have suggested, are merely not perfectible ; but they are not " irrational ". They differ from some calculations about physical probabilities chiefly in degree of complexity, but have analogies with these and with operations of going to see for oneself. In the third place, if a party to a moral dispute refuses all appeal to criteria of equity and utility, then there is no dispute : he neither agrees nor disagrees. Fourthly, if appeal is made at all, then the matter can be pressed. Fifthly, this analysis at least gives sense to the making, within the bounds of

modesty and tolerance, of categorical moral pronouncements ; for it specifies reasonable and acceptable procedures. Sixthly, a person may merely refuse to be consistent ; but there is nothing that can be done about that anyway. Seventhly, a person may apply criteria and yet be in no way prepared to act conformably. But this is the situation, which any analysis has to cope with theoretically, as some unfortunate people have practically, of the moral delinquent : religion and practical psychology may be of use, but it is not surprising if moral philosophy is helpless. At all events it is a problem of giving, in a specific case, a *purpose* to moral discourse, and not of giving it *sense*.

Finally, there is, so it seems to me, a disparity between the amount of the discussion that has been devoted to the allegedly moral use of words like " ought " and " good ", and the extent to which these words are actually used in specifically moral discourse. They have, of course, a very extensive non-moral employment : this has been investigated with subtlety and acuteness by the very philosophers whose analysis of the moral use I have been criticising. But it is not merely that, as Hare himself has argued, the jobs they do in their moral employment *could* be done by other words but that as a matter of fact they usually are. Discussions of such things as the free pursuit of ends do not become moral through the use of these obscuring words. Further, if their use is really " advisory " they are better dropped in moral contexts. For " advice " is not always welcome. If a discussion reaches no conclusion advice is ineffective ; and if it does reach a conclusion it is not necessary.

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II.—THE AUTONOMY OF MORALS

By David Rynin

It is a widely held view that morals are, or at least ought to be, autonomous. Some contemporary and recent thinkers consider the view to have something like axiomatic self-evidence, while others are prepared to offer a variety of arguments in support of it. It is possible to put the position into a fairly perspicuous form by distinguishing between normative and factual statements and then asserting that no normative, hence moral, statement either entails or is entailed by any factual statement. For our discussion it will be assumed that factual and normative statements are fairly well recognizable, the former by being verifiable or falsifiable, or possibly confirmable or disconfirmable, by reference to ascertainable matters of fact, say observable circumstances of our inner life of feeling or of the outer world accessible to sense perception ; the latter by the occurrence in them of certain terms, or their synonyms, or their antonyms, such as " good ", " right ", " ought ", " duty ", whose utterance is accompanied by feelings of moral approbation or disapprobation.

The doctrine of the autonomy of morals might seem to admit of a modified, weaker, form. We might, for instance, be willing to grant that some normative statements entail factual statements, without thereby necessarily committing ourselves to the view that any factual statement entails a normative statement. Many, in fact, hold that " I ought " entails " I can ", hence that some normative statements entail some factual statements, without necessarily agreeing that the converse holds ; while others accept the view that if we have given a promise, then we ought, at least under certain conditions, to keep it, without thereby wishing to commit themselves to the view that any normative statement entails any factual statement. We might thus attempt to distinguish between full autonomy (or heteronomy, its negation) and quasi autonomy (or heteronomy) according to the following scheme, in which N represents any normative statement and F represents any factual statement :

(1) N is fully autonomous $\longleftrightarrow$ N neither entails nor is entailed by F,

(2) N is quasi autonomous $\longleftrightarrow$ N entails but is not entailed by F,

(3) N is quasi heteronomous $\longleftrightarrow$ N is entailed by but does not entail F,

(4) N is fully heteronomous $\longleftrightarrow$ N entails and is entailed by F.

However, there are good reasons to hold that this attempt to provide a modified form of the doctrine of autonomy of morals cannot be carried out. For, granting what appears to be the case, that the negations of normative statements are normative statements and the negations of factual statements, factual statements, and accepting the ordinary logic of propositions (letting the symbol " e$\rightarrow$ " represent " entails ") it can be shown that :

(1) $(N \text{ e}\rightarrow F) \rightarrow (\sim F \text{ e}\rightarrow \sim N)$, and

(2) $(F \text{ e}\rightarrow N) \rightarrow (\sim N \text{ e}\rightarrow \sim F)$.

I shall assume, therefore, that it is a sufficient condition for showing the falsity of the doctrine of the autonomy of morals to show either that some moral statement entails some factual statement or that some factual statement entails some moral statement. I shall ignore the more difficult question as to whether every moral statement entails some factual statement or every factual statement entails some moral statement. There would appear to be no intuitive grounds for holding the latter view, but the former is perhaps rather more plausible.

In order to bring the discussion down to earth and make it relevant to some actually asserted view I shall deal with the doctrine of the autonomy of morals in the context of an argument presented by P. H. Nowell-Smith in his interesting recently-published book *Ethics* (Penguin Books, London, 1954). In chapter 3, entitled " Intuitionism ", Nowell-Smith undertakes to show (p. 36-37) in what sense intuitionism is right in its argument against naturalism, and in what sense wrong. I am not interested here in his arguments against intuitionism, which rest mainly on the view that moral statements, being practical in nature or use, are neither true nor false, and hence *a fortiori* not intuitively true. I shall confine myself to his argument in support of intuitionism's refutation of naturalism, which he accepts. I quote his position at some length, for it exhibits the view I wish to discuss in this paper.

" The strength of intuitionism lies in its uncompromising insistence on the autonomy of morals. To put the point briefly and in my own way, practical discourse, of which moral discourse is a part, cannot be identified with or reduced to any other kind of discourse. Ethical sentences are not, as Moore so clearly shows, psychological or metaphysical or theological sentences.

Almost all earlier theories had tended to reduce ethical concepts and sentences to those of some other subject, usually psychology ; they tried to define words such as ' good ', and ' ought ' in terms, for example, of the satisfaction of desire or of pleasure and pain. Against all such attempts the intuitionists produce *a crushing argument*[1] which is derived (surprisingly) from Hume." Here follows the well-known passage from Hume : *Treatise*, Book III, Part 1, section i :

" . . . In every system of morality which I have hitherto met with I have always remarked that the author proceeds for some time in the ordinary way of reasoning, and establishes the being of a God, or makes observations concerning human affairs : when of a sudden I am surprised to find, that instead of the usual copulations of propositions, *is* and *is not*, I meet with no proposition that is not connected with an *ought*, or an *ought not*. This change is imperceptible ; but is, however, of the last consequence. For as this *ought* or *ought not* expresses some new relation or affirmation, it is necessary that it should be observed and explained ; and at the same time that a reason should be given for what seems altogether inconceivable, how this new relation can be a deduction from others that are entirely different from it."

Nowell-Smith then continues :

" Freely translated into modern terminology, what Hume means is this. In all systems of morality we start with certain statements of fact that are not judgements of value or commands ; they contain no moral words. They are usually statements about God or about human nature, that is to say about what men *are* and *in fact do*. We are then told that *because* these things are so we ought to act in such and such a way ; the answers to practical questions are deduced or in some other way derived from statements about what is the case. *This must be illegitimate reasoning, since the conclusion of an argument can contain nothing which is not in the premises, and there are no ' oughts ' in the premises.*[1]

It is the last, *underscored*, statement that I wish to examine now, in order to see whether this alleged logical principle can indeed support the " crushing argument " delivered against those who in one way or another deny the autonomy of morals by holding that moral statements can be derived from factual statements. Taking for granted here that entailment is the (or at any rate *a*) relation that validates derivation, that is that *S'* is derivable

[1] My italics, D.R.

from $S \longleftrightarrow S$ e$\rightarrow S'$, I ask : Is it the case then, or a principle of logic, that no conclusion of a valid argument can contain anything that is not in the premises, hence that no normative statement is derivable from a factual statement ?

The view that the conclusion of an argument can contain nothing that is not in the premises, while often enough asserted, has I fear very little plausibility, at least in the sense apparently intended by Nowell-Smith. For we note that immediately after uttering this dictum he adds : " . . . and there are no ' oughts ' in the premises ". Presumably the conclusion of a valid argument must contain no terms not appearing in the premises. Such plausibility as the view has is, apparently, derived from the theory of the syllogism. But it really is a bit late in the day to restrict logic to that fragment of itself. And the moment one considers non-syllogistic reasoning one sees that the dictum, far from expressing a principle of logic, is utterly erroneous. The argument : " A is taller than B, therefore B is shorter than A " is surely no less valid than any syllogistic argument, even though " shorter than A " is not contained in the premise. The intuitive self-evidence of the transitivity of the relation of class inclusion that validates *Barbara* can be matched by many other equally self-evident axioms or rules of inference,— a fact amply testified to by modern logic, and evident enough even if modern logic were silent on the matter. We must assume therefore that the crushing argument requires a sounder base than that offered by Nowell-Smith and those who think, or at any rate speak, as he does.

This is not to deny, of course, that some relation must hold between the premises and the conclusion in a valid argument, even if it is not that of containing the terms present in the conclusion. This required relation is in fact that of strict or necessary implication, which we understand in the sense that S necessarily implies $S' \longleftrightarrow S$. $\sim S'$ is self-contradictory —a sufficient condition for which being specifiable in terms of the familiar truth tables. There are reasons, however, for not wishing to base derivability on necessary implication, for as is well-known, it is a sufficient condition for a statement S necessarily to imply another S' if either S is analytically false or S' is analytically true. And hence if it were sufficient for S' to be derivable from S for S' to be necessarily implied by S, we should be able to derive any statement whatever from a self-contradictory one, and this seems not to accord with at least certain intuitions regarding what is meant by " derivable ". One wishes to be able to derive, say, " A is a parent ", from " A has a son ", but certainly not from

every self-contradiction. I shall therefore attempt to formulate the meaning of " entails " in such a manner as to justify the view that if S entails S', then S' is derivable from S. The relation of entailment is therefore stronger than that of necessary implication, at least in the sense I attach to " entails ".

The equivalence :

S entails $S' \longleftrightarrow$ [(S necessarily implies S') and neither S nor S' is analytic] comes about as close to expressing what I wish to understand by " entails " as I can come, speaking in this more or less formal manner. Speaking in terms of content, I should try to explicate " entails " as follows :

S entails $S' \longleftrightarrow$ a necessary truth condition (ntc) for S is a sufficient truth condition (stc) for S'.

Alternatively, assuming that the sense of " entails " is relatively clear compared with that of " stc " and " ntc ", I should attempt to explain the sense I attach to the latter as follows—it being understood that truth conditions whether necessary or sufficient are, for me, facts, *i.e.* states of affairs, or however we wish to designate those circumstances whose existence or non-existence we take as establishing contingent statements as true or false :

(1) that X is a stc of $S \longleftrightarrow$ that X e$\rightarrow S$ is true ;

(2) that X is a ntc of $S \longleftrightarrow$ that $\sim$X e$\rightarrow S$ is false.

Thus in terms of the above I understand the condition that A has a son to be a stc of " A is a parent ", and the condition that A has a child to be a ntc for " A is a parent ".

Accordingly, I shall hold that S' is derivable from $S \longleftrightarrow$ a stc for S' is contained among the ntc's for S. This formulation will perhaps enable us to explain why some statement R, logically independent of another statement S, is not derivable from although necessarily implied by the self-contradiction S . $\sim S$. The reason is that none of the ntc's for the statement S . $\sim S$ are stc's for the statement R. And the formulation will likewise enable us to locate something that must, in a sense, be contained in the pre-mises of a valid argument in which the conclusion is derivable from the premises, namely a stc for the conclusion. I say " in a sense ", for of course it is only in a sense that if a stc for a state-ment S' is contained among the ntc's for a statement S, then there is something contained in S' that is contained in S. I hope that in so speaking I do not abuse the term " contained in " ; but if so, it should be recalled that this is due to a pious attempt to salvage an ancient doctrine. Perhaps such reverence is out of place here, so let us get on with our subject.

Our problem is whether any normative statement either entails

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or is entailed by any factual statement.   The crushing argument that is supposed to show that the derivation of, say, a normative from a factual statement is impossible is thus seen to rest on a misunderstanding of what is required to render the derivation possible.   What Nowell-Smith and all those who agree with him on the doctrine of the autonomy of morals are required to show is that no normative statement either entails or is entailed by any factual statement.   For all we have demonstrated thus far, this view may of course still be correct, although not on the grounds criticized.   How then does one go about showing that the doctrine of autonomy is true, or is false ?   One way might be that of stipulation.   One simply settles the matter by deciding to use the expressions " normative statement" and "factual statement" in such a manner that it is a sufficient condition for some statement S not to be a normative statement for it to entail any factual statement, and a sufficient condition for showing that some statement S is not a factual statement to show that it entails some normative statement.   But two can play this game, and it is not a very interesting one.   At the beginning I formulated the notions of normative and factual statement in quite a different manner, such that it is an empirical, testable, question whether the doctrine of autonomy is correct.   I shall take the liberty of retaining these formulations ; they are rather less question-begging than some others.

Adopting the formulation of the notion of entailment suggested above, we might try to answer our question regarding the thesis of autonomy by attempting to discover whether we can find a pair of statements, one factual and one normative, such that among the ntc's of one there may be found a stc for the other. If so, we shall have demonstrated the falsity of the autonomy thesis, at least if the assumptions mentioned earlier are granted— and no argument will hold except on the basis of some assumptions that make it intelligible and testable.

I observed earlier, and it is well known, that some people hold the view that " I ought " entails " I can ", and others that " I have given a promise " entails, under certain conditions, " I ought to keep my promise ".   In fact, most people hold *many* views similar in nature so far as entailment of factual by normative or normative by factual statements is concerned.   In saying this I do not mean to assert that most people use the word " entails " or have ever heard it used, but that they would agree, say, that no one is under any obligation to do what he cannot do, and that generally speaking, barring special circumstances, one is under an obligation to keep one's promises.

Assuming that some people do believe that such entailment relations hold, how do we test the correctness of their beliefs ?

It seems to me that there is only one answer to this question : We attempt through investigating the speech and action of the users of statements to ascertain whether as they understand and use them it is possible that, say, the normative statement N could be true even if the factual statement F were false, or that F could be true even if N were false. I shall not attempt to spell out the exact procedures for carrying out this kind of investigation, but shall simply affirm that we can and do on the basis of certain tests arrive at well-confirmed hypotheses regarding what given persons hold to be ntc's and stc's of at least some of their statements, and that when the person happens to be one's self, one has knowledge on this matter of a very high order of reliability. I affirm, in brief, that we regularly do arrive at very highly confirmed hypotheses as to what people do accept as ntc's and stc's for their statements, and that among these highly confirmed hypotheses we find many that tell us that some normative statements are used in such a manner as to entail certain factual statements, and conversely,—but if by chance we never do gain any such knowledge, it is clear that no one is ever warranted in saying that no N entails or is entailed by any F.

At this point one may expect to hear the following sort of rebuttal : An argument in which some normative statement appears to be derived from a factual statement is in fact an enthymeme, has a missing premise that is itself a normative statement; hence that argument is valid only if among the premises whether actual or implicit we already have a normative statement. Thus one might say that implicitly contained among the premises of the argument : " I have given my promise, therefore I ought to keep my promise " is the premise " All promises (of a certain kind, let us say) ought to be kept ", which is essential for the validity of the argument and which is not itself derivable from any factual statement. For the sake of the discussion let us suppose that this view can be successfully defended. Would this show that our refutation of the doctrine of autonomy is erroneous ? I think not. The conditional corresponding to this argument will be of the form : $(N \cdot F) \text{e} \rightarrow N'$. But by the principle of exportation this can be transformed into $F \text{e} \rightarrow (N \rightarrow N')$, in which from the factual statement alone we can derive the normative conditional. Thus if we have the argument : " I have given my promise, and all promises ought to be kept, therefore I ought to keep my promise ", we can transform it into " I have given my promise, therefore if all promises ought

to be kept, then I ought to keep my promise." If one accepts
the principle of exportation, then one must accept the view
that one can derive a normative from a factual statement alone
if one can derive it from the conjunction of a factual and a norma-
tive statement—unless of course one is prepared to show that a
conditional statement whose antecedent and consequent are
normative statements is itself not a normative statement. I do
not assert that one cannot make out a case of some sort for this
view, but it certainly does not go without saying. And it would
seem that in so far as one can make out a plausible argument in
support of denying normative character to such a conditional,
one can equally well deny normative character to a conditional
whose antecedent is a factual statement and whose consequent is a
normative statement. If such are not normative but factual
statements, then of course we can show that a normative state-
ment entails a factual statement. For if $N \cdot F \text{ e} \rightarrow N'$, then
$N \text{ e} \rightarrow (F \rightarrow N')$. But we saw earlier that if $N \text{ e} \rightarrow F$, then
$\sim F \text{ e} \rightarrow \sim N$; so nothing has been gained by this effort. I shall
not consider the possibility that $F \rightarrow N$ and $N \rightarrow N'$ are neither
normative nor factual, since I find myself beyond my depth here.

It seems to me, in any case, an error to assume that in an argu-
ment in which a normative statement appears to be derived
from a factual statement, the argument is an enthymeme with a
missing normative premise. What is required to validate
the derivation of a normative from a factual statement is not a
normative premise implicitly conjoined to the factual premise,
but a correct rule of inference *according to which* the inference is
drawn. When we recognize an argument in *modus ponens* as
valid, this is not because the necessarily true conditional corres-
ponding to the argument is a premise in the argument, but
rather because with those premises and that conclusion it is
impossible for the premises to be true and the conclusion false ;
it is this impossibility that validates the argument with those
premises and that conclusion. The validating principle of an
inference is not itself a premise of the argument it is used to
validate. Indeed, it is a commonplace of modern logic to dis-
tinguish between rules of inference and premises of arguments.
The infinite regress that is involved in making rules of inference
into premises was clearly and amusingly exhibited by Lewis
Carroll in " What the Tortoise said to Achilles ".

And now we may expect to have the ground of the argument
shift again. It may be held that what they really want to say—
those who claim that no normative statement may be derived
from a factual statement—is that every such valid derivation

presupposes some normative principle that serves as a rule of inference to validate the derivation. Without this principle the argument will not go through ; but the principle itself is certainly not derived from any factual statement—and this is the essential point !

To this new form of the autonomy thesis we answer that the rule of inference validating a derivation is a kind of criterion or touchstone by means of which we determine whether an argument is of the kind called " valid ". It is not itself in the logical sense derived or inferred from other statements, whether normative or factual, but gradually emerges into our awareness as suitable for determining the validity of an argument. Rules of inference are, in fact, frequently expressed in permissive sentences that are, in the current jargon, *performative* in character. When I say : " You may do so and so ", I am not, when granting permission, making a prediction or ascribing some power or tendency to you. The sentence is not functionally a statement at all, although the same sequence of words sometimes is a statement. We arrive at a rule of inference by becoming aware of the fact that we under-stand certain statements in such a manner that if the one is true the other must be true, that is, by ascertaining, in the case of entailment, that a ntc of a certain statement is a stc for some other. As we do not infer a rule of inference from anything, we do not of course infer it from any factual statement, but neither do we infer it from some normative statement. The rule of inference that validates a derivation is not a premise, nor a statement of any kind ; it cannot be a part of any argument or be presupposed by any argument. All it does is to *validate* the argument, provided the argument is of the required form.

Having, I hope, disposed of the obvious rebuttals to my refuta-tion of the autonomy thesis, I turn to my final point. I do not claim here to have shown that some normative statements are fully reducible to factual statements, or factual to normative. I claim only to have shown that some Ns are partially reducible to some Fs and that some Fs are partially reducible to some Ns. My argument was directed primarily against those who maintain that no F entails any N, and if successful has demonstrated as well that some N entails some F. This is not, of course, to have shown that there exists any particular F that entails some particular N, *and* that *that* particular N entails *that* particular F. But it ought not to be inferred from this that I think there are none, for I think that there are many ; but that is another story. However, since part of it can be briefly told, I shall devote a few words to telling it.

In terms of the above analysis, two statements are fully reducible one to the other if and only if they have identical necessary and sufficient truth conditions. The question of full reducibility then becomes that of determining whether there exists a normative and a factual statement that have identical nstc's. This will be, according to my view, if there exist at least one person who so interprets them. Now I think there can be little doubt on this point. Being unable here to give detailed empirical evidence in support of this view, I shall offer instead a kind of indirect proof, which runs as follows :

(1) *Assumption* : there exists at least one adherent of Christian morality ;

(2) *Quasi definition* : X is an adherent of Christian morality → X judges a person Y to be a morally good man ←→X believes that Y loves his neighbour as himself ; *i.e.* for X, " Y is a morally good man " entails and is entailed by " Y loves his neighbour as himself ".

Given these premises, the conclusion follows that there is at least one person for whom some normative statement is fully reducible to some factual statement. The definition is, I think, not implausible ; and if any one doubts that there is such a person, I am prepared to cast modesty aside and offer myself as such, at least in some moments.

It would be unrealistic to suppose that what has been said here will convince those who reject this whole way of dealing with these questions, who think that the entailment relations in which statements stand have nothing to do with the truth rules their users assign to them, and who for reasons I am unable to fathom think it desirable that the realm of values be independent of matters of fact. But I should like to think that it might have a slight tendency to open up once again a question that is believed by many to have been long since settled, and that in particular it might persuade those who disagree with my conclusions to make clear to themselves what they mean by "entails" and "derivable," and just what if any evidence would be relevant to the determination of the truth or falsity of the thesis of the autonomy of morals.

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III.—TRUTH AS APPRAISAL

By Alan R. White

I wish to examine the notion of truth in a way similar to the recent examinations of the notions of goodness and probability by Mr. R. M. Hare[1] and Professor J. N. Findlay respectively. The plausibility of supposing that the notion of truth can be explained in a way similar to that of goodness derives from the history of the treatment of these notions. Together with beauty, truth and goodness have always been considered to occupy a special and similar place in speculation. They are the eternal values ; and often when an objective or subjective or emotive theory has been held of one it has been held of the others, and often when ' good ' has been rejected as superfluous or non-descriptive or meaningless, so has ' true '.

I hope to show (a) that my results hold not only for those cases where ' true ' and ' false ' are used of statements—or beliefs, opinions, views, propositions, sentences, since it is not important for my point to arbitrate on the question as to what it is that is true or false (cf. Austin, pp. 111-115)—but also for those cases, not usually considered, where ' true ' and ' false ' are used of objects— or nouns, expressions, etc. ; (b) that my view is able to reconcile the various well known opposing theories of truth, such as Coherence, Correspondence, Pragmatism, by showing that they are correct and compatible answers to a different question from that of the meaning of truth ; (c) how some logical positivists are misled into their view that ' true ' and ' false ' are logically superfluous words by their supposition that if an adjective does not describe, it does nothing.

My thesis is that the function of the word ' true ' (or ' false ') is to *appraise* whatever it is used of, whether statement or object. Like all appraisal words, it has two aspects, a descriptive and an evaluative. The descriptive, which may be said to give the *criteria* for saying that anything is true, varies with each kind of statement and each class of object to which it is applied, while the evaluative, which may be said to give the *meaning* of ' true ', remains invariant. For instance, we use ' true ' and ' false ' of various kinds of statements, empirical and analytic statements, scientific hypotheses, perhaps even moral judgements, and we

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[1] References are to the bibliography at the end of the paper.

use them of objects so varied as friends, Conservatives, Englishmen, freedom, temperance, steel, likeness—all of which are commonly said to be ' true '—and of teeth, nose, manner, front,— all of which are commonly called ' false '. The descriptive force of ' true ' and ' false ' varies in all these cases but the evaluative force is the same. For example, to say that a person is a ' false friend ' is both to grade him low and to suggest that he is unreliable, hypocritical, deceitful, etc. ; to say that a mathematical statement is true is to mark it high and to suggest that it fits in with a system of other statements.

That the two aspects of truth are independent of each other may be seen from the following points which are compiled and adapted from the remarks on goodness made by Hare in chapters 6 and 7 of his book. (a) The *criteria*, that is the descriptive meaning, for ' true ' may differ in each class of statement or object yet the word may have the same meaning, that is evaluative *meaning*. So teaching the use of the word ' true ' for different classes is neither a completely new lesson, nor, in the way it is for an ordinary descriptive adjective like ' red ', the same one repeated.

(b) We could know the *criteria* on which ' true ' is in fact used in a given class, *e.g.* Englishman or scientific hypothesis, and yet not know the *meaning* of ' true '. To give the *criteria* of truth is not to give the *meaning*.

(c) On the other hand, we may not know the *criteria* for ' true ' in a given class, *e.g.* Englishman or scientific hypothesis, and therefore the use of the word conveys no information about the characteristics of the object or statement ; and yet we can know the *meaning*. We can sum up (b) and (c) by saying that the descriptive and evaluative aspects of ' true ' are related in such a way that (1) to say ' X is true ' where the criteria of truth are known conveys information about X, (2) to say this where the criteria are not known fails to do so.

(d) The word ' true ' is what Hare calls ' supervenient ', that is, it is sensible to say, for example, that two statements are similar in all respects except that one is in Latin and the other not, but nonsense to say that they are similar in all respects except that one is true and the other not.

(e) The *meaning* may be used to change the *criteria* by using ' true ' for different criteria from those for which we formerly used it. For example, the twentieth century idea of a ' true Conservative ' may differ from the nineteenth century idea, and yet ' true ' means the same. A modern mathematician's idea of a true statement in mathematics may have reference to its

coherence with the system to which it belongs (*cf.* Hempel), whereas that of a layman or traditional mathematician may refer to its application to physical objects, and yet ' true ' *means* the same.

Of course I do not lay any stress on the words ' criteria ' and ' meaning ' here. Both may be called part of the meaning of the word ' true ', so long as the two aspects, the descriptive meaning (or criteria) and the evaluative meaning (or meaning) are distinguished. There is nothing wrong in saying that ' true Conservative ' does not *mean* today what it did in the nineteenth century ; what would be wrong would be to fail to see that, in another sense, it means the same (*cf.* Hare, pp. 109-110, 114). Furthermore, when a Socialist and a Conservative call the same man a ' true Conservative ', they may mean the same in the sense that they are both using the same criteria and that they are both evaluating, but what they mean is different in the sense that the latter is expressing a favourable attitude towards him and the former an unfavourable. ' True ' nearly always expresses a favourable attitude, but of course it can, like ' good ' and many other words, become derogatory in certain contexts.

In many other ways it has been realised before now that a purely descriptive interpretation of ' true ' and ' good ' will not do. Those logical positivists, *e.g.* Ramsey and Ayer, who have said that a sentence formed by adding ' true ' or ' good '—in grammatically appropriate places—to a given sentence does not say anything more than is said by the given sentence, mean that these words do not describe. Moore's objection to the ' naturalistic fallacy ', if used for ' true ' rather than ' good ', shows that it is always sensible to ask whether a statement which fulfils the criteria suggested as the meaning of ' true ', *e.g.* correspondence, coherence, pragmatism, is indeed true. This is a form of argument which Moore himself used against James' pragmatist criterion of truth. It has also been said that the function which the words ' good ' and ' true ' have, since they are not descriptive, is emotive. This is a very common view about ' good ', and has been recently (*cf.* Savery) applied to ' true '. Further, Ryle perhaps alludes to an evaluative element of ' true ' without mentioning whether or not there is also a descriptive element when he says that " there is something of a slur in ' false ' and something honorific in ' true ' ". Professor Barnett Savery has recently suggested the possibility of two elements in our notion of truth, whose differences from mine I shall consider below. Professor Woozley mentions two questions about truth, namely, (i) what does the truth of a belief consist in ?   (ii) how can we test the claim of a belief to be true ?   Now question (ii) corres-

ponds to my descriptive meaning, but I do not understand what exactly question (i) is. Woozley says it is about the 'nature of truth' and that "in any case in which we could discover the nature to be present we should have tested the claim of the belief concerned to be true. In that sense an answer to (i) will also be an answer to (ii)." Quite clearly question (i) does not correspond to my evaluative meaning. In fact it seems to express a desire for a *general descriptive* characteristic of 'true' and is thus open to the 'naturalistic fallacy' argument (*cf.* Hare, pp. 97-103). Mr. Strawson has not only argued against a descriptive view of 'true' but has also suggested a large number of non-descriptive uses. I think that he has made a point similar to mine in his warning (*P.A.S.* p. 145, *cf.* p. 156) that we must distinguish the questions "When do we use the word 'true'?" and "How do we use the word 'true'?". But I have not, I think, seen it advocated before that the correct interpretation of 'true' is that it is both descriptive and evaluative in the way that I have described.

In different contexts it may be either the descriptive or the evaluative force which is stressed. In an argument, especially on political, moral, or aesthetic subjects, and in some reviews and critiques it is only too commonly the case that the descriptive force of 'true' and 'false' in their application to statements is almost absent, just as it is almost absent in their application to objects in such phrases as 'true temperance', 'false friend', 'false manner', 'true Englishman'. On the other hand, in a sober review or a balanced estimate of a particular piece of scientific work and often in daily use the descriptive force is strong in its application to statements, just as it is strong in its application to objects in such phrases as 'false teeth', 'true likeness'.

Although I have called 'true' (and 'false') an appraisal word, and therefore one having both an evaluative and a descriptive function, I am inclined to agree with a point made to me by Hare that the descriptive meaning is, in certain contexts, tied more securely to it than to other appraisal words, *e.g.* 'good'. This is not, I think, important where 'true' and 'false' are used of objects, because although there are cases, as I have already instanced, where the descriptive element is emphasised to the almost total exclusion of the evaluative, there are cases where the opposite is the case, and besides, this is also a characteristic of 'good' and 'bad'. Even with some kinds of statements this situation does not arise. But with statements of an empirical kind like 'The cat is on the mat' it is plausible to say, in accordance

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with the verification principle, that to know its meaning is to know the criteria for its truth, and therefore, that to know the meaning of ' It is true that the cat is on the mat ' is to know the criteria for *its* truth.   In this respect ' It is true that the cat is on the mat ' is like ' That is a good corkscrew '—or any example of a functional word qualified by ' good '—in a way that it is unlike ' He is a good man '.   That is, to know the meaning of ' That is a good corkscrew ' puts us in possession of most of the criteria for its truth whereas to know the meaning of ' He is a good man ' does not.   On the other hand, I am inclined to think that this characteristic of truth is due more to its accompaniment —that is, ' empirical statement '—than to itself, and therefore I am still doubtful if it is correct to say that there are cases where knowledge of the meaning of ' true ' entails knowledge of the criteria of truth.   But, whatever the answer here, the close connexion of the notions of ' statement ' and ' truth ', must, as I point out below, be kept always in view.

Another possible objection to my view—which I owe to Mr. W. V. Denard—is that words used evaluatively have degrees of comparison, form a grading scale, whereas ' true ' and ' false ' are not so obviously used in this way.   Now one thing we have to notice immediately is that we do in fact quite often use ' true ' and ' false ' comparatively, as in ' a truer account or description ', ' far from the truth ', ' it would be truer to say ', ' not quite false ', etc.   Of course it might be said that such uses should be analysed in such a way that we mean not that one thing is more true or false than another but that it contains more or fewer parts, for example, sub-statements, which are entirely true or false. But however they are analysed we do use ' true ' and ' false ' in a comparative way, and I think we do it precisely because we wish to evaluate to a certain degree, wholeheartedly or hesitantly, polemically or politely ; we wish to rank one thing above or below another in this respect.   But to admit the comparative use of ' true ' is not of course to subscribe to degrees of truth in the Idealist philosopher's sense.   If we insist on denying a comparative use of ' true ' we could parallel this with other evaluative words, like ' right ', which have no degrees of comparison.

If we consider some well known theories of truth as used of statements, we can, I believe, see that they were wrongly supposed to be answers to the question '' What is the meaning of truth ? '', whereas they are in fact answers to the question '' What are the criteria of truth ? ''.   That is, they are about the descriptive, not the evaluative function.   That they were not

answers to the question " What is the meaning of truth ? " was seen by some logical positivists, who wrongly concluded that there was no legitimate question about the meaning of truth but only about its criteria. For instance, Ayer, says " there can be no logical problem concerning the nature of truth " ((1), p. 238) since " the purpose of a ' theory of truth ' is simply to describe the criteria by which the validity of the various kinds of propositions is determined " ((2), p. 87).

Apart from the Correspondence view, which has certain peculiarities to which I shall refer later, the well known theories of truth can all, I think, be shown not to be about the meaning of truth, by using Moore's ' naturalistic fallacy ' argument, as he himself did, though without calling it such, in his article on ' Professor James' Pragmatism ' (*P.A.S.*, 1907-8). It is not self-contradictory to say that a statement works (Pragmatism) or coheres with other statements (Coherence) or is supported by authority or is intuited, etc., but is false. It is always sensible to ask whether a statement which works, coheres, etc., is true. But none of these would be possible if pragmatism, coherence, etc., gave the *meaning* of ' true ', that is, the evaluative function.

But these theories have a relation to truth ; and that is that they, each in particular types of case, furnish the criteria, serve the descriptive function, of truth. The criterion for deciding whether a logical or mathematical, that is an analytical, statement is true is that of coherence within a system (*cf.* Schlick, Hempel ;) the criterion for deciding the truth of a scientific hypothesis is pragmatic ; the criterion for deciding the truth of an empirical statement is correspondence with the facts. The criterion for deciding the truth of a moral judgement is possibly something different again. If we remember that the descriptive function of truth is as much one of its functions as the evaluative, and in that sense is part of its meaning, and if we remember also that the descriptive meaning, that is the set of criteria, varies for different types of statement—as for different classes of objects—we can, I think, explain why these theories seemed plausible even as theories of the meaning af truth. They seemed plausible because each concentrated originally on one type of statement ; they were, in Wittgenstein's phrase, reared on a one-sided diet. The correspondence theory fitted empirical statements, the coherence theory fitted mathematical and logical statements, and the pragmatist theory fitted scientific hypotheses. They were correct so long as they confined themselves to their respective types of statement and so long as only the descriptive function of truth was in question.

Besides furnishing the various criteria, that is the descriptive meaning, of truth, there is another reason for the appropriateness of each of these theories to a particular kind of statement. If we ask ourselves what is an empirical statement, what is an analytic statement, what is a scientific hypothesis, and how are they distinguished from each other, then at least one way to answer this question is in terms of the varying ways we prove each kind to be true or false. So that it is part of the meaning of ' empirical statement ' that its criteria of true and false are correspondence, as of ' analytic statement ' and ' scientific hypothesis ' that their criteria are coherence and pragmatism respectively. Similarly, part of the difficulty about the nature of moral judgements is to discover what criteria we have in mind when we call them, as we naturally and correctly do, true or false. All this shows, I think, why it sounds queer, if not self-contradictory, to say any of the following ; " This mathematical statement coheres with the system of mathematical statements in which it occurs, but it is false " ; " This scientific hypothesis works completely, but it is false " ; " This empirical statement corresponds with the facts, but it is false."

Because it has been usual to stress, or even to be aware of, only the descriptive use of language, and therefore to suppose that all statements are empirical, the correspondence theory has usually conquered. Indeed this theory is nowadays felt to be a truism, as in fact it is in regard to empirical statements, since it is part of the meaning of ' empirical ' (cf. Strawson, *P.A.S.*, pp. 141-143). And the theory is sometimes turned into a truism for other statements by the flexibility of the word ' fact ' (cf. Austin, pp. 117-118, Cousin, pp. 168-169, Strawson, *P.A.S.*, pp. 133-143), which starts by being used in connexion with those things, that is events, occurrences, situations, etc., which could be said to be described by affirmative empirical statements, and then is extended to cover what negative, conditional, general, etc., statements state. For this reason I think that it is not quite satisfactory to consider the correspondence view as a *theory* in the same sense as the other views are theories about truth. Because of the flexible use of ' fact ', ' correspondence with the facts ' becomes a way of referring to the descriptive meaning not only of ' empirically true ', but even of ' analytically true ', ' hypothetically true ', ' morally true ', etc. Even if it were correct to universalise the correspondence theory in this way, it would not deprive ' true ' of its evaluating meaning, but would only show that the reasons or criteria we have for evaluating things in this way, that is calling them true, are the same in every case, namely, corres-

pondence with fact (reality, etc.). Now the fact that it is obviously incorrect that the reasons for calling something true are the same for every class of *object. e.g.* Conservative, freedom, likeness, would in itself, I think, throw doubt on the view that they are the same for every class of *statement*. And I have above given other arguments which purport to show that the reasons or criteria are not indeed the same for every class of statement, unless ' fact ' is made a blanket, and hence useless, term.

I think that this double function of the notion of truth makes some sense of James' pragmatist views. As is well known, his theory of truth seemed plausible as a general theory only because the key phrases, such as ' what works ', ' suitability to a purpose ', were made to cover such diverse things as ' verification ', ' consistency ', ' making life worth living ', ' satisfaction '. James wished, and in a sense he was right (*pace* Austin, pp. 125-127), to use ' true ' about things as diverse as empirical and mathematical statements, scientific hypotheses, moral judgements, and religious and metaphysical systems, and yet he realised that the criteria for judging these to be true were vastly different. His solution was to give such a flexible meaning to ' truth ' that is, ' suitability to a purpose ' or ' satisfaction ', that it could be used to cover all these varying criteria. In this respect it was similar to the traditional attempts to explain the meaning of ' good ' in functional terms, that is that something is good if it fulfils its purpose. All these attempts at least saw that a theory of truth or goodness had to account for the fact that ' true ' or ' good ' had in some sense the same meaning in all uses ; and they erred only in thinking that this was a common *descriptive* meaning.

It is when a theory seeks to universalise its criteria and apply to more than its own kind of statement that it errs. So the notorious difficulties that the Correspondence theory has with conditionals, negatives, universals, laws, etc., are usually met either by regarding facts as very queer sorts of things, as for instance Russell and Moore, among others, did at one time (*cf.* Woozley's phrases, ' hypostatised event ', ' conveniently intelligible aspect of the event '), or by using ' fact ' in a sense so different from the use originally intended with affirmative descriptive statements that it is almost a synonym for truth. Similar fantasies are bred by idealist philosophers like Leibniz, Spinoza, Bradley, who try to force all statements into the criteria of truth characteristic of analytic statements, and by the pragmatists who force all into the criteria characteristic of hypotheses.

The two-fold function of ' true ' also, I believe, helps us to

understand the view of some logical positivists, *e.g.* Ramsey and Ayer, that the word ' true ' is logically superfluous. This arose from the belief, emphasised in Wittgenstein's *Tractatus Logico-Philosophicus* (*e.g.* 4.01, 4.016, 4.021, 4.03, 4.031, 4.462) that an expression does not ' say anything ' unless it describes. When it was realised that neither ' good ' nor ' true ' described in the sense that most adjectives do, it was said that they added nothing to the statements in which they occurred, and they were called ' logically superfluous ' (Ayer (2), p. 88) and even meaningless (Ayer (1), p. 238, ' never adds anything to the sense '). A good example of this approach can be seen in Ayer's *Language, Truth and Logic* by juxtaposing a sentence from his argument on ' good ' and one from his argument on ' true '. " When, for example, one says that the proposition ' Queen Anne is dead ' is true, all that one is saying is that Queen Anne is dead ". " Thus if I say to someone, ' You acted wrongly in stealing that money ' I am not saying anything more than if I had simply said ' You stole that money '."[1] Here Ayer considers the words ' good ' and ' true ' logically superfluous because by adding them we do not ' say anything ', do not describe, more than is already said by a statement which does not contain them. Indeed he must have considered his argument stronger in the case of truth than of goodness because he sought some other function for ' good '—*e.g.* ' evincing my moral disapproval ', ' express feeling '—while he seems to have felt no need for this in the case of ' true '. Ramsey (p. 142) suggested that ' true ' and ' false ' were " phrases which we sometimes use for emphasis or for stylistic reasons, or to indicate the position occupied by the statement in our argument ". Ayer now (*Revue Internationale de Philosophie*, vol. vii (1953)) seems to abandon his view in those cases where the statement which is said to be true is not actually mentioned, while retaining it where the statement is mentioned. He still argues for the conclusion that ' to speak of a sentence, or a statement, as true is tantamount to asserting it . . .".

The correct element in this view—of which Ayer's version is only one example—namely that the function of ' true ' is not purely descriptive, occurs again in a confused form in the view commonly held, especially in opposition to the semanticists, that a statement to the effect that a statement is true does not *say* anything *about* the original statement. That is, that what the meta-statement and the statement say are the same, *e.g.* that

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[1] A further fault, not relevant here, in the second quotation is the use of ' stealing ' and ' stole ' which already contain an ethical force. This could be avoided by using ' taking ' and ' took '.

Queen Anne is dead. This view is quite right if it only means that the meta-statement *describes* nothing in addition to what is described by the original statement, in somewhat the same way as ' He walked into my room and stole the money ' might be said not to describe anything additional to what is described by ' He walked into my room and took the money '. But it was falsely inferred from this that such a meta-statement does not *say* anything at all that is not said by the original statement, and also that it is not *about* the original statement. I have tried to show that the meta-statement does say something additional, namely, it makes an appraisal, though of course it does not describe. It is certainly about the original statement, though perhaps not in the same sense that the original statement is about Queen Anne or about her death. It is about it in the common and natural sense in which this word is used in any example like the following. " ' That's very true ', says A. ' What are you talking about (what are you referring to) ? ' says B. ' This statement in the paper (or C's last remark) ', says A." That is, a statement can be about, or refer to, a statement without describing anything. ' True ' and ' false ' are used for ' characterising somebody's statement ' although they do not give a ' property ' of it (Ryle). Strawson has advocated the view that while it is wrong to suppose that to use the word ' true ' is to make a statement about something, it is correct that it is to do something additional to making the original statement. I agree with the latter part of this view, and I think that I agree with the substance of the former, but I am not sure what exactly Strawson is saying. His chief reason for denying that to use ' true ' is to make a statement about anything is that it is not used to make a statement about a *sentence*. While I agree with him that ' true ' is not used to make a statement about a sentence and therefore is not a meta-statement in this sense, I do not see that it follows from this that it does not make a statement about anything. Perhaps the point here is merely terminological, since, although Strawson does say that " the phrase ' is true ' . . . is not *applied* to anything " and that it " *never* has a statement-making role ", what he means, I think (*cf.* pp. 268, 270, 273, 274, 277) is that it does not describe or assert anything. I am concerned to show that ' true ' is used to evaluate statements and objects, and that in this—not merely a ' grammatical ', but yet, I think, a ' philosophically non-committal ' (*cf.* Strawson, *P.A.S.*, pp. 145-148)—sense it is *about* statements and objects ; and in this sense it applies to something, although it does not describe. Whether evaluations are to be called ' statements ' or not, and

whether they are to be said to make ' assertions ', and therefore whether my view commits me to saying that ' true ' is used to make a statement about something, is, I think, a point of no importance.

Woozley (pp. 173-174) says that in *most* cases the phrase ' is true ' does nothing that is not done by the statement itself, but he feels that in *some* cases it ' dispositionally asserts ' or ' asserts a second order proposition '. But I cannot see that ' dispositionally asserting ' and ' asserting a second order proposition ' are, as they seem intended to be, descriptions of the same function ; nor can I understand in what way ' p is true ' could be said to ' dispositionally assert ' p. His view of what the second order proposition does, namely, ' calling attention to the identity of a proposition and a fact which constitutes a proposition's truth ', seems, in its most likely sense, to suggest that ' true ' is used to make a point of logic or linguistics ; and I find this incredible.

Savery has recently suggested a distinction between a specific meaning which might be said to correspond to my *criteria*, and a generic meaning, which might be said to correspond to my *meaning*, for the notion of truth. The former can take the form of any of the traditional theories of truth, while the latter is said to be ' End of Enquiry '. The chief differences (*cf.* p. 520) between Savery's views and mine are that (*a*) his element of ' End of Enquiry ' is different from my evaluative meaning, and indeed seems to be another example of a general descriptive meaning somewhat like James' ' suitability to a purpose ' ; (*b*) Savery allows that both the meaning, that is, End of Enquiry, and the criteria may change ; (*c*) he often collapses his two elements into one, *e.g.* " Plato's end of enquiry, hence Plato's specific meaning of truth (1.1) rational intuitionism, is a criterion which will bring an end to enquiry " ; " Those who accept faith as the criterion of truth, and argue that this is the end of enquiry ". And he calls ' verifiability ' both an ' end of enquiry ' and a ' criterion '. (*d*) Most importantly, whereas I have related the various traditional criteria to the various types of statement, Savery says " our basis for accepting one criterion of truth rather than another depends on some will-attitude, some interest, some arbitrary fiat, something that is describable in non-rational rather than rational terms ". He suggests psychological reasons why we accept one criterion rather than another, whereas I suggest logical reasons, that is reasons related to the type of statement which we are appraising. I do not of course wish to deny that there are all sorts of psychological reasons why people

accept something as true or think it is true, but I wish to say that
these have nothing to do with the logical reasons for thinking it
true or with the reasons why it is true. Religious faith may
be a psychological reason for accepting an empirical state-
ment as true, just as reading it in *The Times* may be,
but ' verifiability ' (p. 520), is a logical reason since it is bound
up with the meaning of ' empirical ' and of ' truth ' as descriptively
related to this kind of statement. To say that someone read it in
The Times or accepted it on religious faith, but it is false, is not
queer or self-contradictory even if it is never true ; whereas it is
queer to say that he verified it but it is false. Perhaps however
Savery means either that there may come a time when our
conceptual framework will so change that ' empirical ', ' veri-
fiable ', ' analytic ', ' coherent ', etc., and ' true ' will not mean
what they now mean ; or that our interests may shift, like Keats',
so far to the aesthetic that we will no longer be interested in the
distinction we now make between truth and beauty ; and that
in this sense our use of ' true ' is arbitrary and non-rational. I
do not wish to dispute this. I agree that, in the way I mentioned
earlier, the meaning of ' true ' may be used to change the criteria.
for example, of ' true ' as used of Conservative and as used of
mathematical statement ; and I agree that people do make
irrational as well as rational evaluations, and that it is an in-
teresting question as to *why* they do this.

It should be added that the fact that ' true ' is used evalua-
tively, and not purely descriptively, does not, any more than in the
case of ' good ' (*cf.* Hare, pp. 108-109) or ' probable ' (*cf.* Findlay.
221-222, 228-233), entail that it is subjective or relative in any
derogatory sense, and of course it does not entail that it is descrip-
tive of a state of mind. There can be good and bad reasons for
asserting that something is true or good. Judgements of value
of any kind may be reasonable or unreasonable, right or wrong,
considered or hasty, well-backed or groundless. An appreciation
of the evaluative element of truth no more implies that one
must consider an ill-informed man's views on what is true as
being as valuable as those of the well-informed, than a similar
appreciation of a similar element in the notion of goodness implies
that one cannot rationally choose between Hitler's moral judge-
ments and those of Christ or Dr. Ewing (*cf. The definition of good*,
chs. i and ii).

I have been concerned only to point out the double function
of ' true ' and ' false ', namely, the descriptive and evaluative.
This is perhaps not to do very much unless some detail is given
of what kinds of jobs are evaluative, and how the evaluative

jobs done by ' true ' and ' false ' differ from those done by ' good ' and ' bad ', ' logical ' and ' illogical ', etc.   Hare has done this to some extent for ' good ' and Strawson for ' true '.   And although there are differences between ' good ' and ' true ', there is, I think, some similarity in their evaluative functions. ' True ' as well as ' good ' praises, directs, guides, encourages, agrees, endorses, expresses a favourable attitude, sets up standards. Perhaps it is mainly this last which was the concern of most theories of truth, and in a sense was rightly their concern since the kinds of standard point to the kinds of evaluation.

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IV.—THE CONTRADICTORY FUNCTION

By T. A. Rose

I

THE propositional calculus or calculus of " unanalysed " propositions is generally concerned with establishing certain relations, *e.g.* implication or equivalence, between expressions containing propositional variables such as p, q, r . . . and constant functions such as $\supset$, $\mathbf{v}$, $\bullet$ and $\sim$. The latter are often described as the "implicative", "disjunctive", "conjunctive" and " contradictory " functions. I shall try to show that, strictly speaking, the contradictory function $\sim$ does not belong to this group and that, if homogeneity is to be preserved, it should be replaced by another function.

All these are called " material " or " adjunctive ", or " truth-functions ", in order to distinguish them from the " formal " or " connective " functions dealt with in other calculi. The epithets, " material ", " adjunctive " and " truth ", are supposed to indicate that whether one of these functions holds depends solely on the truth-value(s) of the proposition(s) it has as argument(s).

The fact that $\sim$ differs from the other functions is manifest, first of all, in logicians' use of the phrase ' *the* contradictory of p' when interpreting $\sim$ p[1]. The use of the definite article indicates that $\sim$ p is meant to signify a single proposition, or that, once p is replaced by a concrete proposition, $\sim$ p is uniquely determined : *e.g.* if p is *Socrates is human,* $\sim$ p is *Socrates is not human.* It would be inappropriate to prefix the definite article to such phrases as ' implicans of p ', or ' disjunct of p ', or ' conjunct of p ', because any number of propositions answer to each of these descriptions : *e.g.* if p is the true proposition *Socrates is human,* any proposition will be an implicans or disjunct of p, and any true proposition will be an implicate or a conjunct of p.

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[1] *Cf. Principia Mathematica,* 1st edn., p. 6 : " The Contradictory Function with argument p, where p is any proposition, is the proposition which is the contradictory of p, that is, the proposition asserting that p is not true. This is denoted by $\sim$ p. Thus $\sim$ p is the contradictory function with p as argument and means the negation of the proposition p." I shall generally use the terminology of *Principia Mathematica,* but the subsequent discussion applies to most expositions of the propositional calculus. The expression ' formal ' will be used to distinguish relations and functions which depend upon the internal structure of propositions they have as arguments : ' formally implies ', *e.g.*, will indicate an entailment, not a formal implication in Russell's sense.

However, since it is possible to find propositions formally equivalent to the contradictory of p, the point about uniqueness might be thought unimportant.   For example, if p is *Socrates is human*, *Socrates is non-human* is equivalent to p's strict contradictory, *Socrates is not human* ; so, if we use ' contradictory ' in a more general sense, it will be possible to speak not only of ' the implicants of p ', ' the disjuncts of p ', etc., but also of ' the contradictories of p ', and it will be inappropriate to prefix the definite article to the phrase ' contradictory of p '.

But even if we use ' contradictory ' in this extended sense, there is still a fundamental difference between contradiction and the other functions.   First, the relation of p to any of its contradictories is fundamentally different from the relation of p to any of its implicants, disjuncts, etc.   This is evident from the fact that a modal is always appropriate to the relation between p and any of its contradictories.   For example, if p is *Socrates is human*, then whether $\sim$ p is *Socrates is not human* or *Socrates is non-human*, we may say not merely that p and $\sim$ p are not both true and are not both false, but that they cannot both be true and cannot both be false.   In the case of the other functions, we are never entitled, without further information, to use a modal.   For example, the relation between p and any one of its disjuncts is a relation such that either p or its disjunct is true, and not necessarily a relation such that either p or its disjunct is necessary, even though as exemplifications of this function we may have *Socrates is human or Socrates is not human* (where a modal would be in order), as well as *Socrates is human or Plato is a Greek* (where a modal is not permissible).

Second, there is still a fundamental difference between the relation of p's contradictories to one another and the relation of p's implicants, p's disjuncts, etc., to one another : p's contradictories are formally equivalent to one another, whereas p's implicants, disjuncts, etc., are not always even materially equivalent to one another.   For example, supposing p is true, then, while its implicates are materially equivalent and its conjuncts are materially equivalent, its implicants are not and its disjuncts are not, because a true proposition is implied by all propositions, true or false, and the disjunction of any proposition and a true proposition is true.

The point I want to emphasise is the first.   The contradictory function of the propositional calculus is a formal function, and, in so far as it occurs in expressions like ' the contradictory of p ', it is a special case of this formal function, *viz*. the one that occurs in the traditional square of opposition.

II

There is a function, however, which is material, and which, by analogy with the formal function, could be called "the (material) contradictory function". It is a function such that a proposition and any one of its contradictories are not both true and are not both false, or have opposite truth-values. We are never entitled to say that a proposition and one of its contradictories, in this sense, cannot both be true and cannot both be false, unless we have further information. Obviously there are any number of propositions with a truth-value opposite to that of any given proposition. So, a proposition will have any number of material contradictories just as it has any number of material implicants or material disjuncts ; and it will be no more appropriate to prefix the definite article to ' contradictory of p ' when ' contradictory ' has this sense than to prefix the definite article to ' (material) implicans of p ' or to ' (material) disjunct of p '. For example, if p is the true proposition *Socrates is human*, not only *Socrates is not human*, but *Plato is a Persian, Aristotle is an Egyptian*—in fact, any false proposition—will answer to the description ' contradictory of p '. These will all be materially equivalent, but only some of them will be formally equivalent to one another. They will all be material contradictories of p, but only some of them will be formal contradictories, and only one will be *the* formal contradictory, of p.

I shall express the relation of material contradiction by ' ∥ ', so that ' p ∥ q ' will mean *p and q are not both true and are not both false,* or *have opposite truth-values.*[1] The corresponding relation of formal contradiction I shall express by ' —— ', so that ' p——q ' will mean *p and q cannot both be true and cannot both be false.* The expression ' ∥ p ', derived from ' p ∥ q ', will mean *material contradictory of p* or *proposition with a truth-value opposite to that of p*, while the expression ' ——p ', derived from ' p——q ', will mean *formal contradictory of p* or *proposition which must have a truth-value opposite to that of p*. The function ∼ p of the propositional calculus is a species of ——p.

III

In order to clarify the relation of material contradiction I shall now set out all the possible material relations after the manner in which logical relations, *i.e.* formal or modal relations between propositions, can be set out in the traditional

[1] The material relation ∥ is the product of the disjunctive function **V** and the stroke function **|**, ' p | q ' meaning *not both p and q*. Łukasiewicz has ' *J* ' for ' ∥ '.

logic, borrowing some of the traditional terminology and giving it a material interpretation.

In an exposition of the logic of the four forms of proposition, A, E, I, and O, where all the propositions considered are assumed to have genuine terms for subject and predicate, it can be shown that, given any two propositions with the same terms in the same order, one and only one of five relations may hold between them, *viz.* those of the square of opposition [1] ; while, given any two propositions with any genuine terms whatsoever, in any order, one and only one of seven general relations may hold between them. If the two propositions are p and q, the seven general relations are : p is equivalent to q, p is in superaltern relation to q, p is in subaltern relation to q, p is in contradictory relation to q, p is in contrary relation to q, p is in subcontrary relation to q, p is indifferent to q.[2]

The contradictory function of the calculus of propositions is derived from the formal relation of contradiction : from the relation of the square of opposition, if the expression ' the contradictory of p ' is permitted ; from the general relation, if it is not.

The material relations one and only one of which holds between any two propositions are the four called " specific " in the following table :

Material Relations

p q	General	Specific
T T	(1) p and q are equivalent	(3) p and q are subcontraries
F F		(5) p and q are contraries
T F	(2) p and q are contradictories	(7) p is a subaltern of q
F T		(9) p is a superaltern of q

(i)   (ii)   (iii)

[1] " Symbolic " logicians, because of their belief that there can be an exclusively extensional or exclusively intensional logic, succeeded for a long time in throwing this doctrine into disorder : *cf.* Lewis and Langford, *Symbolic Logic*, ch. 3. H. L. A. Hart (' A Logician's Fairy Tale ', *Philosophical Review*, April 1951) and P. F. Strawson (*Introduction to Logical Theory*) argue cogently against them.

[2] There are various alternative terminologies : especially ' equipollent ' for ' equivalent ' and ' independent ' for ' indifferent '. *Cf.* Johnson, *Logic*, Part I, p. 49, and Stebbing, *Modern Introduction to Logic*, 7th edn., ch. v (1). There is an important parallel between the seven " complex propositions " of De Morgan's *Formal Logic* and the seven " logical relations " between propositions.

The numbering (1), (2), (3), etc., corresponds to that in the tables of binary material functions and monary material functions given in the next section. Granted that there are material functions, the binary functions which are omitted here, *viz.* (4), (6), (8) and (10), are the " formal contradictories " of (3), (5), (7) and (9) respectively. They must be true for those values of the arguments for which the latter are false, and false for those values for which the latter are true. Material relations are strictly binary ; unless a convention about grouping is observed, some of the binary functions may have more than two arguments.

Column (i) sets out the truth-possibilities for any two propositions p and q. Column (ii) divides these into two cases : the case in which p and q have the same truth-value and are said to be materially equivalent, and the case in which p and q have different or opposite truth-values and are said to be materially contradictory. The terminology is derived from that for the corresponding formal relations. If p and q must be either both true or both false, they are formally equivalent : in the present case p and q *are* both true or both false. If p and q cannot be either both true or both false, they are formally contradictory : here p and q *are* not both true or both false.

Column (iii) divides each of these general cases into two sub-cases and contains the material relations one and only one of which holds between any two propositions. Here the justification of the terminology is not as obvious as for column (ii). However, since the first two relations are species of material equivalence, *i.e.* since in these two cases the truth-values of p and q are the same, it is clear that the names we choose must suggest symmetrical relations. Of the remaining formal relations which are symmetrical, those which have requirements analogous to those of the two species of material equivalence are sub-contrary relation (p and q may both be true and cannot both be false), which is analogous to the first case of material equivalence (p and q are both true, or neither p nor q is false) ; and contrary relation (p and q cannot both be true but may both be false), which is analogous to the second case of material equivalence (neither p nor q is true, or p and q are both false).

For the sub-cases of material contradiction there is no great difficulty in finding suitable names, because these are asymmetrical relations, and the only asymmetrical relations among the formal ones are superaltern relation and subaltern relation. So, for the case in which p is false and q is true (p materially implies q but q does not materially imply p) we may say p is a material superaltern of q (where the requirements for the corresponding

formal relations are : if p is true, q must be true, but if q is true, p may be false) ; and for the case in which p is true and q is false we may say p is a material subaltern of q.

By an examination of the truth-possibilities, then, it can be seen that any two propositions, p and q, exhibit one and only one of the relations in column (iii), and that, no matter which of these relations holds, it will be an instance of one of the general relations in column (ii), *i.e.* of material equivalence or material contradiction. The most obvious differences between the material relations and the formal relations are that every formal relation has more than one truth-falsity requirement, that among the former there is no relation of indifference and that the material relations of equivalence and contradiction are not on the same level as the other material relations.

Of the genuine material functions commonly grouped together as simple functions only conjunction and the dagger function (p ↓ q meaning *neither* p *nor* q) appear in the table as specific relations, *viz.* as material subcontrariety and material contrariety respectively. They differ from the other material functions, including equivalence and contradiction, in holding for only one set of truth-possibilities : p true and q true, and p false and q false respectively. The specific relations, p is a material subaltern of q and p is a material superaltern of q would commonly be expressed as $\sim (p \supset q)$ and $\sim (q \supset p)$ respectively, *i.e.* as denials of implications rather than as simple functions. Implication and disjunction do not appear as specific relations because they hold for more than one set of truth-possibilities of their arguments. They do not appear as general relations because the features of the (three) sets of truth-possibilities for which they respectively hold do not lend themselves as readily to generalisation as do those for which material equivalence or material contradiction holds.

So far I have tried to show that the contradictory function of the propositional calculus is not a material function but a rather special formal function ; that there is a genuine material contradictory function ; that this genuine material contradictory function is the "formal contradictory" of material equivalence. This suggests that, while the treatment of functions in the calculus is heterogeneous, it could be homogeneous.

IV

It might be thought that the foregoing discussion depends on various confusions : failing to take account of the distinction between monary and binary functions, missing the point about

variables, running together what is taken for granted outside the calculus and what happens within the calculus. I shall try to show that the confusions are on the other side.

The genuine distinction between the contradictory function as formal and the other functions of the propositional calculus as material is obscured by another distinction commonly made between them. It is commonly stated that the contradictory function is " monary " and that the other functions are " binary " What is meant is that the contradictory function has only one argument or requires that only one argument place be filled by a proposition, while the other functions have several arguments or require that at least two places be filled by a proposition. By way of clarification we have to see that there are various distinct but interrelated ways of understanding not only contradiction but also the other functions or operations :

(A) Contradiction may be regarded as a relation between propositions, *i.e.* as an independent function which takes at least two propositions as arguments. So regarded, it would resemble implication, disjunction and conjunction, when they are treated as independent functions of propositions, in being a binary function. Here we should have to distinguish, as already intimated, between material binary functions and formal binary functions. In so far as a homogeneous interpretation were given to the functions of the propositional calculus, the interpretation we should naturally put upon ' p contradicts q ' is p materially contradicts q, or p and q have opposite truth-values, *i.e.* p ‖ q, not p——q. The most important binary material functions are defined in the following table :

Binary Material Functions

p	q	p=q	p‖q	p●q	p|q	p↓q	p∨q	~(p⊃q)	p⊃q	~(q⊃p)	q⊃p
T	T	T	F	T	F	F	T	F	T	F	T
T	F	F	T	F	T	F	T	T	F	F	T
F	T	F	T	F	T	F	T	F	T	T	F
F	F	T	F	F	T	T	F	F	T	F	T
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)

The successive pairs of functions, p=q and p ‖ q, p ● q and p | q, etc., are pairs of " formal contradictories ". Analogous binary formal functions would be : (1) p and q must be both true or both false (formal equivalence), (2) p and q cannot be both true or both false (formal contradictory relation) ; (3) p and q may both be true (formal consistency), (4) p and q cannot

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both be true (formal inconsistency) ; (5) p and q may both be false, (6) p and q cannot both be false ; (7) if p is true, q may be false (p does not entail q), (8) if p is true, q must be true (p entails q) ; (9) if q is true, p may be false (q does not entail p), (10) if q is true, p must be true (q entails p). These are all pairs of formal contradictories except the first pair, which are contraries. I mention them mainly for the sake of distinguishing them from the material functions and, so far from admitting that they are in any way subsidiary to the material functions, would deny that there is even a close analogy between the two types of function. This is borne out by the fact that whereas in the previous section, for the sake of arriving at a suitable nomenclature, it was convenient to compare the material relations (3), (5), (7), and (9) with the formal relations of subcontrariety, contrariety, p is in subaltern relation to q and p is in superaltern relation to q respectively, it now seems just as plausible to compare them with the more general formal relations of consistency, consistency of contradictories, p does not entail q and q does not entail p. The explanation of the equal plausibility of these comparisons is that in the case of material relations there is no way of making a distinction between subcontrariety and consistency, between contrariety and consistency of contradictories, between p is a subaltern of q and p does not imply q, or between p is a superaltern of q and q does not imply p.

(B) Our interest in contradiction may be an interest in the contradictory or the contradictories of a proposition, *i.e.* in a proposition determined by its having, or propositions determined by their having, the relation of contradiction to a given proposition. Here we are still concerned with an independent relation or binary function of propositions, but we are interested in only one side of the relation, or in the other side only as determining this side. The relational expression or monary function, ' contradictory of p ', is of the same order as, for example, the relational expressions, ' implicans of p ', ' implicate of p ', ' disjunct of p ' and ' conjunct of p '.

Obviously each of these is based on the corresponding binary function. We must distinguish just as carefully between material monary functions and formal monary functions as we did between the corresponding binary functions. In particular, *material contradictory of p*, expressed as ' || p ' and based on p || q, is to be contrasted with *formal contradictory of p*, derived from p——q and expressed as ' ——p ' (*cf.* $\sim$ p). The material functions, implicans of p, implicate of p, disjunct of p, conjunct of p, etc., could be expressed as ' ⊃ p ', ' ⊂ p ', ' ∨ p ', ' ● p ', etc.

The most important monary material functions are defined in the following table :

Monary Material Functions

p	=p	‖p	●p	|p	↓p	∨p	∼⊂p	⊂p	∼⊃p	⊃p
T	T	F	T	F	—	T/F	F	T	—	T/F
F	F	T	—	T/F	F	T	—	T/F	T	F
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)

Analogous monary formal functions—omitting, for the sake of brevity, the qualification ' formal ' or ' formally '—would be : (1) proposition equivalent to p, (2) proposition in contradictory relation to p ; (3) proposition consistent with p, (4) proposition inconsistent with p ; (5) proposition whose contradictory is consistent with that of p, (6) proposition whose contradictory is inconsistent with that of p ; (7) proposition not entailed by p, (8) proposition entailed by p ; (9) proposition which does not entail p, (10) proposition which entails p. Each of these pairs of formal functions determines mutually exclusive, and exhaustive, classes, except the first pair, which determines classes which exclude one another, but which together do not exhaust the whole class of pairs of propositions. These formal functions are in no way subsidiary to the material functions.

One striking feature of the monary material functions is that, with the exception of material equivalent of p, =p, and material contradictory of p, ‖ p, either the question of the falsity of the argument does not arise, e.g. ● p and ∼ ⊂ p, or the question of the truth of the argument does not arise, e.g. ↓ p and ∼⊃ p ; or, when the argument may be true or false, there is one truth-possibility which leaves the truth of the whole function indeterminate. But in every case, even when the truth of the whole function is determinate for every truth-possibility of the argument, the whole function does not signify a unique proposition.

Independent monary and binary functions, especially the formal functions of contradiction, are the root or basic notions from which the ones I am about to consider are derived.

(C) Contradiction may be regarded as denial. So understood it is of the same order as affirmation, and means a person's operation on a proposition rather than an independent function of a proposition, even though this " mental operation " would be impossible unless there were the corresponding independent functions. The consistent application of this view to the calculus would require that we have a function ' — ' such that ' — p ' is

to mean *p is denied*, just as '⊢ p ' is sometimes taken to mean *p is affirmed* or *p is asserted* ; and, since we are concerned with unanalysed propositions, denial, like affirmation, would be a monary function. Further, unless the functions of the calculus are to be heterogeneous in this respect, we should have to treat the other functions as mental operations too, although, like denial, they would be impossible if there were not the corresponding independent functions. But since denial depends on the formal function of contradiction, to group it with the material functions would still prevent the calculus from being thoroughly homogeneous.

(D) Contradiction may be regarded as the affirmation or assertion of the contradictory or the contradictories of a proposition. Admittedly, it ordinarily means the affirmation of the formal contradictory of a proposition, ⊢ ∼ p. But to continue with our examination of the possibilities, the appropriate expressions would be ' ⊢ ‖ p ' for the affirmation of the material contradictories of p, and ' ⊢ —— p ' (*cf.* '⊢ ∼ p ') for the affirmation of the formal contradictories of p. If we are to proceed in this way with contradiction, there is no reason why we should not do so with the other functions, having, *e.g.*, a device for affirming the material implicants of p. The corresponding expressions for the affirmation of the other material functions, implicans of p, implicate of p, disjunct of p, conjunct of p, etc., would be ' ⊢ ⊃ p ','⊢ ⊃ p ', ' ⊢ ∨ p ', ' ⊢ ● p ', etc. All these operations would be monary.

But, as already pointed out, contradiction of this order is ordinarily taken to mean the affirmation or assertion of the formal contradictory. So understood, its inclusion in the calculus of propositions makes the calculus heterogeneous.

(E) Contradiction may be regarded as the assertion of a proposition's falsity. It then seems to be of the same order as the assertion of a proposition's truth. We might use the expressions ' Tp ' for *p is true* and ' Fp ' for *p is false*. If we allowed these expressions, we should have to describe them as monary functions. The expression ' Fp ' depends for its meaning on the monary formal function, and ultimately on the binary formal function, of contradiction. So this view provides no escape from heterogeneity.

(F) Contradiction may be regarded as par excellence a relation holding between *p is true* and *p is false*, *i.e.* between Tp and Fp. Here the relation would be formal.

Other notions could be added to those already mentioned : *e.g.* ' It is not true that ', ' It is not the case that ', ' It is false that '. The foregoing discussion applies to them too.

V

I see no important use for most of the expressions which emerged in the last section. My main object was to show that so long as we are concerned with the implicative, disjunctive, conjunctive, contradictory, etc., functions as independent functions of propositions, there is for each a basic binary function, from which one or, in the case of implication, two monary functions may be derived. This is so for both material and formal functions. There is nothing pre-eminently monary about the contradictory function or pre-eminently binary about the other functions.

Nevertheless, in expositions of the calculus of propositions it is often stated categorically that contradiction is a monary function and that the other functions are binary. The description of the function $\sim$ as monary helps to conceal its formal character, because it conceals the fact that it is a derived function. Once it is seen to be derived, it becomes apparent that, of the two binary contradictory functions from which it could have been derived, it has been derived from the formal one. Also, the description of the other functions of the calculus as binary obscures the fact that these binary functions give rise to monary functions of a logical type different from that of the contradictory function of the calculus but similar to that of the genuine material function of contradiction.

If they want to treat the functions of the propositional calculus homogeneously, it is only on the " mental operation " interpretation (C) that calculus exponents can justify the distinction between contradiction as essentially monary and the other functions as essentially binary. The other functions then become the operations of materially " inferring ", disjoining, conjoining, etc., each of which requires at least two propositions as arguments; and contradiction, like affirmation or assertion, then seems to be an operation requiring only one proposition as argument.

Apparently it would be possible to have an operation of materially affirming or asserting, such that ' materially affirming p ' involved not directly affirming p itself but affirming a proposition or propositions with the same truth-value as p, and an operation of materially denying, such that ' materially denying p ' involved denying a proposition or propositions with the same truth-value as p. I hold no brief for either of these " operations ", but one can hardly avoid considering them when seeking an interpretation of the propositional calculus. Clearly material affirmation or assertion does not occur in typical presentations

of the calculus.   What occurs in these presentations is affirmation or assertion in the ordinary sense.   So when calculus exponents think of contradiction as a mental operation, they tend to think of it in its ordinary sense too, *viz.* as denial.

The mental operations (C) would be impossible unless there were the corresponding independent functions (A) and (B). Affirming and denying are commonly thought to be opposed. It is said to be " illogical " to affirm and deny one and the same proposition.   But the logical opposition is not between affirming and denying as such.   The logical opposition, the explanation of the illogicality, is *prima facie* the opposition between *p is true* and *p is false* ; this depends on the relation of formal contradiction, and ultimately on the formal features of the propositions concerned, *i.e.* on their having terms in common, on their terms' having the same order, on the nature and disposition of their quantifiers, on the one's being affirmative and the other's being negative.   Even if the mental operation treatment of the functions provides a *prima facie* case for distinguishing the contradictory function, and the function of affirmation or assertion, as monary from the other functions of the calculus as binary, it does not completely succeed in concealing the formal character of this function.

However, we do not often find even this degree of homogeneity. Affirmation or assertion and contradiction are usually treated quite differently.   While assertion is regarded as a mental operation and given a special sign which may be placed before any complete expression for which truth is claimed, contradiction is not treated as a mental operation at all ; and there is no sign, corresponding to ' ⊢ ' which can be placed before any expression for which falsity is claimed : *i.e.* there is '⊢ p ' but not ' — p ' as I interpreted them in (C).

Assertion is now treated as I treated it in (C), *i.e.* as a mental operation.   Contradiction is now treated, and has been treated by most logicians since *Principia Mathematica*, as I treated it in (B), *i.e.* as *contradictory*, in fact, *the formal contradictory, of a proposition*, an independent monary function.   But whereas we should now expect contradiction to be compared with the other independent monary functions treated in (B), *i.e.* with implicans of a proposition, implicate of a proposition, etc., and contrasted with contradiction as treated in (A), *i.e.* with the binary function of contradiction, it is implicitly compared and contrasted with the other functions treated in (A), *i.e.* with the independent binary functions, except the independent binary function of contradiction.   This engenders the illusion that contradiction, even

when understood as an independent function, is especially monary, while the other functions are especially binary, and discourages the comparison which would reveal its formal character.

When we deny p, we do not simply ignore it or reject it outright : we affirm the formal contradictory of p, as in (D). Reference to the way in which contradiction is treated in (D) is necessary if we want to show what is involved in contradiction as treated in (C), *i.e.* denial. The full and proper meaning of ' p is denied ' seems to be conveyed by ' $\vdash \sim$ p ' or perhaps by ' $\sim$ p ' if we allow that ' p ', though unanalysed, contains a principal verb or has the declarative form. The trouble is that in applying an operator we sometimes feel that we are transforming a proposition into a term. But the other independent monary functions can be treated in this way too. In fact, a homogeneous treatment of functions along these lines would make them all monary, and then there would be no escaping the fact that the contradictory function of the calculus is formal. There is an obvious difference of type between the way in which ' $\vdash \sim$ p ' would be interpreted and the way in which, *e.g.*, ' $\vdash \supset$ p ' or even ' $\vdash \bullet$ p ' would be interpreted if they occurred in the calculus.

Calculus exponents would also render ' $\vdash \sim$ p ' as ' p is false ', *i.e.* in the manner of (E). This way of speaking is unsystematic, because it confuses the " features " of the " objects " upon which the calculus is to be turned as an instrument with the features of the calculus itself. While it has many difficulties of its own, the more systematic way of speaking would be to take propositions, true or false, as the field of operation, and not to have either *true* or *false* as a component in the operations or functions of the calculus itself.

But the former way of speaking also suggests that the contradictory function is an exclusively monary function, because ' is false ', like ' is true ', has all the appearance of referring to an attribute, a one-place predicate, and there is no possibility of subjecting the other functions to this simple treatment. But there is really no attribute *true* and no attribute *false*. We know what ' p is false ' means only if we know what the formal contradictory of p is, and we know what ' p is true ' means only if we know what p is. We do not first find p and then find it being true or being false. We either find that p, *e.g.* that *Socrates is a philosopher*, or we find that $\sim$ p, that *Socrates is not a philosopher*.

' False ' differs from ' true ' in virtue of the fact that whereas saying ' p is true ' is just a pleonastic way of saying ' p ', saying ' p is false ' really amounts to saying that the formal contradictory

of p is true. It is possible to overlook the relation involved in the work done by the expression ' false ', because we think of it as analogous with ' true '. In this way the unsystematic treatment of '⊢ ∼ p ' as meaning *it is asserted that p is false* also lends colour to the view that the contradictory function is monary, and hides its formal character. We forget the formal relation involved in (F), the opposition of Tp and Fp, and think of it as an opposition between the " non-formal attributes ", *true* and *false, viz.* No true are false and No non-true are non-false.

VI

Certain features of variables mislead us into countenancing the interpretation of the contradictory function as a formal function when its truth-table definition, strictly speaking, yields only a material function. The contradictory function of the calculus of propositions is commonly defined in this way :

The Contradictory Function

p	∼ p
T	F
F	T

The propositional function ∼ p is false when p is true, and true when p is false. This is a definition of a truth-function. It is a definition of the monary material function of contradiction. According to this definition, ∼ p is simply a proposition with a truth-value opposite to that of p. There will be any number of these " contradictories " for any proposition p. The expression ∼ p might still be described as ' the contradictory function of p ', meaning *this* function of p as distinct from any other, but if we speak also of ' the contradictory of p ', meaning the unique proposition which any proposition p has as its contradictory, we are going beyond what is sanctioned by the truth-table definition. We are making a decision that, contrary to the definition, ∼ is to be the monary formal function derived from the relation of contradiction which occurs in the square of opposition.

What obscures this point, that if ∼ p is a truth-function of p, *viz.* ∥ p, the function ∼ p which actually occurs in the calculus has not been defined, is the fact that if we compare the ' ∼ p ' of the truth-table and ' p ' we feel that there is a formal relationship here in virtue of the repetition of ' p '. We feel that, even though

$\sim$ p has been defined as a truth-function, there is no essential difference between the relationship of p to $\sim$ p and that of (C) p is affirmed to p is denied, or that of (D) p is affirmed to the formal contradictory of p is affirmed, or that of (E) p is true to p is false.

There is indeed a certain relationship between the $\sim$ p of the truth-table and p, but it is not a relationship that would establish the equivalence, let alone the identity, of $\sim$ p so defined and p is denied or the formal contradictory of p is affirmed or p is false, *i.e.* the equivalence or identity of the $\sim$ p defined in the truth-table and the $\sim$ p which occurs in the calculus. The relationship consists in the fact that ($\|$ p) $\|$ (p) is a tautologous propositional function, whereas, *e.g.* (q)$\|$(p) is not. The first function is true no matter what proposition we substitute for p, whereas the second is true for some values of q and p and false for others.

The propositional function ($\|$ p) $\|$ (p) differs from the ordinary tautologous functions of the propositional calculus. Since $\|$ p does not determine a unique proposition when p is replaced by a concrete proposition, the quantification (p) : ($\|$ p) $\|$ (p) has to be construed ' For *all* values of p, *whatever* is a material contradictory of p, and p, are not both true and are not both false ', whereas, interpreting $\sim$ p in the usual way, (p) : ($\sim$ p) $\|$ (p) can be construed ' For all values of p, the formal contradictory of p, and p, are not both true and are not both false '. Nevertheless, there is a difference between ($\|$ p) $\|$ (p) and (q) $\|$ (p) which could be expressed by saying that the former is tautologous and the latter is not. There would be a similar difference between ($\supset$ p) $\supset$ (p) and q $\supset$ p, and between (v p) v (p) and q v p. But the tautologous character of ($\|$ p) $\|$ (p) is no more reason for taking the truth-functional $\sim$ to be the formal contradictory function derived from the square of opposition than the tautologous character of ($\supset$ p) $\supset$ (p) is for taking $\supset$ to be the formal relation of entailment.

The following objection might be raised. For all this, there is still a strong formal relationship between p and $\|$ p. Even if the function defined in the truth-table is $\|$ p, p and $\|$ p cannot both be true and cannot both be false, just as p and $\sim$ p, the function that occurs in the calculus (the formal contradictory of p) cannot both be true and cannot both be false. Monary functions like $\supset$ p and v p do not occur in the calculus. If they did, we should straightway see that if $\supset$ p is true, then p must be true, and that either v p or p must be true.

That would be to overlook the fact that the force of the modal in ' If $\supset$ p is true, p must be true ' is completely accounted for by the universal quantifier in ' (p) : ($\supset$ p) $\supset$ (p) ', *i.e.* ' For all values of p, if a material implicans of p is true, p is true '.

Similarly, the force of the modal in ' p and ‖ p cannot both be true and cannot both be false ' is adequately conveyed by the universal quantifier in ' (p) : (‖ p) ‖ (p) ', *i.e.* ' For all values of p, a material contradictory of p, and p, are not both true and are not both false '.

The force of ' p and ~ p cannot both be true and cannot both be false ' is not completely conveyed by ' (p) : (~ p) ‖ (p) ', *i.e.* ' For all values of p, the formal contradictory of p, and p, are not both true and are not both false '. There would be no redundancy in writing : ' (p): (~ p) —— (p) ', *i.e.* ' For all values of p, the formal contradictory of p, and p, cannot both be true and cannot both be false '. All that the modal conveys, in the case of the material function, is that the opposition of truth-values holds no matter what proposition is substituted for p. But in the case of the formal function there is an additional formal opposition between the propositions comprising the possible pairs of interpretations of p and ~ p, propositions such as *Socrates is a philosopher* and *Socrates is not a philosopher.*

We must introduce ' interpretation ' in this sense in order to bring out the difference. If we merely substitute a proposition for p in the two cases, the proposition which makes the difference does not appear. Thus, if we substitute ' Socrates is a philosopher ' for ' p ' in ' p and ‖ p ', and then in ' p and ~ p ', we get ' Socrates is a philosopher and ‖ Socrates is a philosopher ' and ' Socrates is a philosopher and ~ Socrates is a philosopher ' respectively. The proposition ' Socrates is *not* a philosopher ' does not appear. But if we try to carry interpretation a step further, from purely logical resources, we find that in the case of ' ‖ Socrates is a philosopher ' we can go no further, while in the case of ' ~ Socrates is a philosopher ' we may go on to ' Socrates is not a philosopher ', or to some *formally* equivalent proposition. This is so because it is understood that ' ~ Socrates is a philosopher ' is to convey not merely the material contradictory function, but the formal contradictory function, of *Socrates is a philosopher.* It is for this reason that the force of ' p and ~ p cannot both be true and cannot both be false ' is not completely conveyed by ' (p) : (~ p) ‖ (p) ' and that there is no redundancy in saying, ' For all values of p, the formal contradictory of p, and p, cannot both be true and cannot both be false '.

It may clarify the matter if I refer back to Sections III and IV, where I described p = q and p ‖ q, as well as p ● q and p | q, etc., as " formal contradictories ". There is no necessity about the relation of p to ‖ p other than this : no matter what p is, p and ‖ p have opposite truth-values. This indeed is just the

definition of ‖ p. There is nothing necessary about the definition But the relation of p = q to p ‖ q has an additional necessity : no matter what p and q are, p = q and p ‖ q must have opposite truth-values. For here we are not merely repeating a definition : we are pointing to a clash between = and ‖ as defined, *i.e.* between having the same truth-value and having opposite truth-values or not having the same truth-value.

The relation between propositions which are possible pairs of interpretations of ' p ' and ' ∼ p ', as the latter function is interpreted in the calculus, is a one-one relation, *viz.* the relation found in the square of opposition. But the relation between propositions which are possible pairs of interpretations of ' p ' and ' ∼ p ', as the latter function is defined in the truth-table, *viz.* as ‖ p, is a one-many relation. The contradictory function which occurs in the calculus is not the function which is defined in the truth-table. Certain puzzling features of variables prevent us at first from seeing this.

VII

The tendency to make a sharp distinction between what is said and can be said about the functions outside the calculus and what happens within the calculus also clouds the issue. It may be stated categorically that the contradictory function of this calculus is monary and the other functions of this calculus are binary. Such a statement is not incompatible with recognizing what is the case, that there is a binary function of contradiction and there are monary functions of implication, disjunction and conjunction, and that the monary contradictory function of the calculus is derived from the corresponding formal binary function.

Such a statement might be tantamount to recognizing, first of all, that there are the binary functions, both material and formal, and the corresponding derived monary functions, both material and formal ; and to deciding, in the second place, that, while there is this homogeneity and symmetry, we are going to use the binary, not the monary, the material, not the formal, functions of implication, etc., and the monary, not the binary, the formal, not the material, function of contradiction, in fact the formal function of the square of opposition. In this way we should make it clear that our calculus is a thoroughly mixed one, and that this is so, not because of the exclusively monary or exclusively binary, exclusively material or exclusively formal character of a function, but because we want the calculus to be so.

Calculus exponents do not always make this clear, and at the same time they would like us to judge their calculi as closed systems with a definite task to accomplish. Altogether, then, there is a strengthening of the impression that the contradictory function and the others differ absolutely in respect of the number of arguments they require. This in turn obscures the fact that the contradictory function of the calculus depends on the corresponding binary formal function and the fact that there is a binary function within the resources of the calculus, *i.e.* a material function, from which a monary contradictory function could have been derived.

VIII

Prima facie the treatment of functions in the propositional calculus could be homogeneous, because there is a material contradictory function. But the cost of substituting this function for the formal contradictory function would be, first, a certain loss of symmetry. In the calculus as it stands ' p ', ' q ', ' r ' . . . and ' $\sim$ p ', ' $\sim$ q ', ' $\sim$ r ' . . . are supposed to refer to unique propositions. The sort of generality which these expressions have has been called " ambiguity ". Apparently, then, it would be upsetting to admit expressions like ' $\supset$ p ', ' $\vee$ p ' and ' $\bullet$ p ' into the calculus. They do not refer to unique propositions. The substitution, for the variable in any of these expressions, of a sentence conveying a specific proposition does not uniquely determine another specific proposition, but at best a specific class of propositions, *e.g.* in the case of ' $\bullet$ p ' the class of true propositions, and sometimes, *e.g.* in the case of ' $\supset$ p ', not even a specific class of propositions.

But if ' $\sim$ p ' meant *material contradictory of p*, as in fact it does if we take its truth-table definition seriously, it would no more refer to a unique proposition than ' $\supset$ p ' or ' $\vee$ p ' or ' $\bullet$ p ' does. It would rather determine a specific class of propositions, *viz.* those with a truth-value opposite to that of p. In the foregoing discussion I have treated ' $\parallel$ p ' indifferently as referring to a class of propositions, as referring indefinitely to a member of that class, and as referring to the class-concept of that class. This did not matter because my object was to show that ' $\sim$ p ' as defined in the truth-table, *i.e.* ' $\parallel$ p ', is on any legitimate interpretation different from ' $\sim$ p ' as it occurs in the calculus.

However, what interpretation to put on ' $\parallel$ p ' would become an important question if ' $\parallel$ p ' were substituted for the formal ' $\sim$ p ' of the calculus. Neither the above-mentioned class nor

its class-concept is itself a proposition. An indefinite member of this class is a proposition, but if we interpret ' ‖ p ' in this way, symmetry is lost. The device of having ' ‖ p ' refer to *the standard material contradictory of p* would remove the indefiniteness and restore the symmetry. Such *ad hoc* measures have been common in logic at least since Boole.[1] But though it might serve some useful purpose as a device, no one could properly hold that there is such a proposition or say what it is, let alone identify it with the formal contradictory of p.

If the function defined in the truth-table, *i.e.* ‖ p, were substituted for the formal function which occurs in the calculus, *modus tollens* would not establish what it is usually taken to establish. For example, there would be a valid inference from ' If Socrates is a Persian then Plato is an Egyptian, and Not-(Plato is an Egyptian) ' not to ' Socrates is not a Persian ' but rather to ' Not-(Socrates is a Persian) ', where the ' Not ' is to be interpreted materially. What we should be entitled to assert if the premises are true, is the material contradictories, or some indefinite material contradictory, or the standard material contradictory, whatever that might mean, of ' Socrates is a Persian '. In order to derive the conclusion ' Socrates is not a Persian ', or its formal equivalent, the formal contradictory function is required.

Yet *modus ponens*, a form of argument equivalent to *modus tollens*, would establish what it is ordinarily taken to establish. From ' If Socrates is a Greek then Plato is a philosopher, and Socrates is a Greek ' we could apparently infer ' Plato is a philosopher '. This is so because affirmation or assertion is not being interpreted materially. The force of affirming ' Socrates is a Greek ' is not simply that of affirming a proposition, or propositions, or the proposition, with the same truth-value as ' Socrates is a Greek '. If it were, this argument would entitle us to assert not ' Plato is a philosopher ', but rather the material equivalents, or some indefinite material equivalent, or the standard material equivalent, of ' Plato is a philosopher '.

It is also worth pointing out that the law of double negation would not convey what it is usually taken to convey. The expression ' ‖ ‖ p ' would not convey the same proposition as ' p ', but rather a proposition with the same truth-value as p. This indefiniteness would spread throughout the calculus.

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[1] *Cf.* ' the null class ', ' the universe class ' (derived from Boole) ; ' the True ', ' the False ' (Frege). For ' the standard true proposition ' and the standard false proposition ', see Prior's *Formal Logic*, i, iii.

How the present discussion affects the relationship of this calculus to the higher calculi would require fuller treatment than is possible here. It would appear that the monary contradictory function which actually occurs in the propositional calculus is of the same type as that which could be derived from the relation of contradiction which is supposed to hold, *e.g.*, between propositions of the forms (x) Px and $(\exists x) \sim$ Px, or of the forms (x) P$x \supset$ Qx and $(\exists x)$ P$x \bullet \sim$ Qx. Propositions such as these cannot both be true and cannot both be false.

There would be no possibility of deriving the formal relation which holds, *e.g.*, between propositions of the forms (x) Px and $(\exists x) \sim$ Px, *via* that between expressions of the forms P$a \bullet$ P$b \bullet$ P$c \bullet \ldots$ and $\sim$P$a \vee \sim$P$b \vee \sim$P$c \vee \ldots$, from that between expressions of the forms p $\bullet$ q $\bullet$ r $\bullet \ldots$ and $\sim$p $\vee \sim$q $\vee \sim$r $\vee \ldots$, if $\sim$ in the latter case were the material contradictory function. Unless we assume that the relation between p and $\sim$p, q and $\sim$q, etc., is one of formal contradiction, there is nothing to show that having substituted Pa for p, Pb for q, etc., in p $\bullet$ q $\bullet$ r $\bullet \ldots$, we should substitute $\sim$Pa for $\sim$p, $\sim$Pb for $\sim$q, etc., in $\sim$p $\vee \sim$q $\vee \sim$r $\vee \ldots$; for $\sim$Pa is not taken to mean just a proposition with a truth-value opposite to that of Pa. It is taken to mean *a is not P*, and must be interpreted in this way in order to make possible the transition to $(\exists x) \sim$ Px.

The upshot is that so far as contradiction is concerned, the propositional calculus is " higher " than the predicate calculus, because it depends upon the predicate calculus. It is interesting to contemplate whether, had they not confused material implication with entailment, and material contradiction with formal contradiction, the original propounders of " modern " logic would not have looked to formal functions rather than material functions for the foundations of logic.

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V.—THE LOGIC OF POSITIVE TERMS AND THE TRANSCENDENTAL NOTION OF BEING

By D. M. Tulloch

In Question I, article 1, of the *De Veritate*[1] St. Thomas Aquinas writes, " That, however, which the intellect conceives first as best known, and in which it resolves all conceptions is being (ens) as Avicenna says in the beginning of his *Metaphysics*, Book I, chapter 9. Therefore, all other conceptions of the intellect must be arrived at by addition to being. But something cannot be added to being as an extraneous nature, in the manner in which a difference is added to a genus or an accident to a subject, for every nature is essentially being, and therefore the Philosopher in the third book of the *Metaphysics* proves likewise that being cannot be a genus." Again, he writes, " For that which first falls under apprehension is *being*, the understanding of which is included in all things whatsoever a man apprehends ".[2]

Thomist writers spend much time and care in explaining exactly what is meant, and what is *not* meant, by the claim that being is the first intelligible, and in drawing out the implications of a philosophy of being in the spheres of metaphysics, theory of knowledge, and theory of value. However, it is perhaps not surprising that these efforts meet with more appreciation from other thomists than they do from philosophers of a different persuasion, for the deceptive simplicity of the initial presentation of this thesis leads immediately and, it would seem, almost inevitably to a highly complicated elaboration, in which the wholesale importation of technical terms may well befog the mind of the listener, and, we may suspect, sometimes that of the exponent himself.

The question therefore suggests itself whether it would not be more fruitful to test the thesis in action by taking a particular question around which there is discussion outside the thomist circle, and seeing whether what the thomist has to say about the concept of being is relevant to this discussion. This is what I

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[1] *Quaestiones Disputatae*, Vol. I, *De Veritate* (Marietti, Turin, 1949).
[2] *Summa Theologica*, I-II, Q. 94, a. 2.

propose to attempt in this paper. I shall not present a systematic account of the theory that being is the first intelligible, but merely indicate, with the minimum of elucidation, the propositions about being which seem to me to be significant in relation to the points I am discussing.

I

The Question of Logical Incompatibility. In his paper, " The Linguistic Theory of *A Priori* Propositions " [1] Dr. Ewing discusses the view that " *a priori* rules of logic are valid because they could not be denied without self-contradiction ", and makes the following objection : " it is impossible to deduce all *a priori* propositions from the law of contradiction alone. For the law to be applied we must be able to see that certain characteristics are incompatible with each other, and from the bare law of contradiction alone one could not deduce which these would be, *e.g.* that red and green are such characteristics and not red and smooth or noisy " (*ibid.* pp. 235, 236). According to Ewing, therefore, before we can use the formal law of contradiction we must be provided with certain information concerning the intelligible content of certain terms, between which contents we *see*, or are directly aware of, the logical relation of incompatibility. Now, it is difficult to understand what is meant by saying that before we can tell whether the law of contradiction applies to two given characteristics we must see that they are incompatible, that we *see* this in the case of red and green, and that we do not *see* it in the case of red and smooth. Furthermore—and this will provide us with a subject for reflection in Section II—we may ask whether, in the second case, we see something else, *viz.* something called " logical compatibility " ? For, in fact, the incompatibility here referred to is not something like incompatibility of temperament as between, for example, husband and wife, or the incompatibility of two colours as when we say that this red blouse " does not go with " this purple skirt, but, precisely, logical incompatibility, and this, we suggest, means the logical impossibility of two terms both being predicated of one and the same subject. If this is so, then Ewing's point is that the law of contradiction is in itself incapable of telling us which terms, considered in their logical function as predicates, are incompatible. But in this case it is of no use to try to make good this deficiency by appealing to whatever it is that those terms really refer to ; for if we simply confine ourselves to looking at

[1] *Proceedings of the Aristotelian Society,* 1939-40.

each of these things, for instance, the objective characteristic of green as such, as distinct entities, then, of each of them, all we can say is that it *is* itself, and that it *is not* any of the others. "Everything is what it is and not another thing ": this, of course, is true, and in this sense it is never the case that the law of contradiction does not apply, but this goes no way towards helping us to discover whether two given positive terms, like red and green, are incompatible. It seems to me that the inspection of whatever it is in the real that the terms are supposed to refer to is irrelevant to the question of the logical incompatibility of these terms ; and I further suggest that when Ewing says that we " see " that red and green are incompatible what he is assuming is precisely that we are viewing these terms as determinates of the determinable, colour, and in their function as determinates, *i.e.* as determining the place of an object, viewed as the possible subject of a judgement, within the order generated by the determinable.

It seems to me that it is only in this context that we can talk about logical incompatibility. If so it is clear that *as such* this logical relationship does not bear on the terms in their reality reference, but on their formal characteristics as determinates placing an object within an order or scheme under a given determinable. This being the case, however, it also seems clear that the law of contradiction *is* sufficient to guarantee the truth of a proposition like " the same thing is never both red and green all over " (*ibid.* p. 214), for the terms red and green are here being viewed in their formal aspect as determinates of a given determinable. Now, if red and green are treated as determinates of the determinable, colour, then their function is to place an object (" a thing ") within the order of colour. If both are employed to characterise one and the same object then this object is at once identified and not identified within the order, which amounts to saying that these terms have ceased to fulfil their function, and this amounts to saying that the order has itself been destroyed.

However, it may be objected that it remains true that the law of contradiction does not itself inform us as to which terms are, and which terms are not, determinates of one and the same determinable. This is, of course, true, but my point is that the question of logical incompatibility and compatibility does not arise until we view terms in their logical function of placing things in orders. If we ask how it is that red and green are logically incompatible, and that red and smooth are logically compatible the answer is that in judging that " X " is red we are viewing " X " exclusively under the determinable colour and

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giving it its place in this order ; it remains an open question whether it has a place in another order—*e.g.* a place signified by the term, smooth. We might still, and rightly, be pressed for an account of how we may be said to discover that two terms are determinates of the same determinable. But here we must be very careful to understand the nature of this question correctly, for both the term " determinate " and the term " determinable " are logical terms, and we must avoid any illicit transposition from the logical order to the real order. We must not seek *in the real* for anything more than is absolutely necessary in order to account for the intelligible (or " useful ", as Ewing's opponents might say) employment of the determinable-determinate category in our knowledge of the real. What is essential in the relationship of determinates to their determinable is the notion of *difference within unity.* The colour red, for example, differs as colour from the colour green. But the phrase " difference within unity " is itself a logical phrase, and to transpose this directly into the real order would involve hypostasising colour and treating it as the real source of the unity, the differences within it being then explained according to some theory of participation. Now, I do not suggest that Ewing considers " colour " to be the name of a real subsistent entity, but it does seem to me that it is in this direction that he would be forced to go if he insisted that it is by inspection of " objective characteristics " that we *see* the logical incompatibility between certain terms. We must consider whether any other course is open to us.

When we are talking of the mind's relation to the real in knowing, it seems clear that, however we may describe this activity, what is distinctive of " knowing the real " is the putting or finding of an order or orders in the diversity of the real as that is presented to us through our senses. This, however, would not be possible if what confronted us were a sheer blank diversity of individual sensa. There must therefore *be* resemblances in the real, and of such a kind that they leave place *within* them for difference. But I use the phrase " putting *or* finding " deliberately, because while the real must be such that we can, as it were " find " differences within unities in it, it is only when these are taken up by the mind and employed as principles governing predication that they can be said to provide us with means for ordering the real. In the real, for example, there are different colours, but from the point of view of explaining logical incompatibility it is sufficient to say that it is by an act of the mind that the positive terms which refer to these different colours are treated as indices of an order, *viz.* the order of colour. Furthermore, it

does seem reasonable to agree so far with Ewing's opponents that there is a certain freedom in the choice of orders—that we do to a certain extent, decide which differences within resemblances we are going to recognise and employ in our " putting of order in the real ". We register this recognition by giving names to these differences, and our motives for recognising the differences we do recognise may vary. Once, however, that we have decided to order our experience according, for example, to colour differences, then a proposition such as " A thing cannot be both green and red all over " is necessarily true, and would be so in any language in which it could be expressed, that is to say, in any language which gives names to colour differences as such, and employs these names as predicate terms of judgements.

II

The Question of Logical Compatibility. The foregoing treatment of the question of logical incompatibility suggested by Ewing's criticism of the Linguistic theory leads me to consider the question of logical compatibility. For, if we take " logical compatibility " to mean the possibility of employing two terms as predicates of one and the same subject without contradiction, then it is clear that in order to describe two terms as logically compatible it is not sufficient, in the light of our treatment of incompatibility, merely to say that the terms in question place objects in different orders. We must be able to say that they place one and the same object in different orders, for, as we have said, two predicates like " red " and " smooth " are logically compatible if they can be predicated of one and the same subject. Thus, " red " and " smooth " are logically compatible if we can say " X ", *e.g.* " This apple ", " is red and smooth ". On my interpretation this means that one and the same object (This apple) must be able to be placed or viewed in the two different orders generated by the determinables, colour, and texture. But it must be noted that this possibility cannot be grounded in either of these two orders, for each of them *is* an order precisely on condition that the object is, by the law of contradiction, identified solely by the determinable concerned. The identity which each of these orders can confer is solely an identity within itself. But when we say that X is both red and smooth it is clear that X cannot be identified solely by " red ", or solely by " smooth ". Hence the identity involved in " one and the same object " cannot be the identity provided for by the particular

orders of the two determinables, whose terms we describe as logically compatible.

The notion of " one and the same object belonging to different orders " necessarily carries us above any one of these different orders and cannot be explained within them : each one of these orders is, as it were, absolute within itself, but, under pain of ruling out the possibility that more than one such order can be employed in identifying a given object, we must conclude that neither singly nor in sum, since each is absolute in its own light, can they themselves contain the ground of the identity, the one-and-the-sameness, of the given object. But here again, to make my position clear, I must insist that the notion of " one and the same object " is, strictly speaking, a logical notion, and that, therefore, what I am arguing is that this notion, which I claim is involved in the notion of logical compatibility, cannot be accounted for within a logic of positive terms, as I have interpreted the latter. There is more to it than this, however, for we must note that if the logic of positive terms serves to mark off objects from each other, so that, for example, if we know that A is red, we know that it is different from B, which is green, it now becomes clear that if none of these orders can be considered the ground of identity of an object which belongs to more than one of them, it is also the case that, while they can each be employed in the task of marking off one object from another, they cannot either singly or in sum be said to contain the ultimate ground of the difference between objects.

My contention is that when we mark off objects from one another we always do so within a unity. I have developed this in terms of the notion of a unity of order generated by any given determinable under which positive terms are grouped, e.g. the determinable, colour, under which we place " red " and " orange ". We can then say " X (This apple) is different from Y (This orange) in so far as X is red and Y is orange "; again, we can say " X is different from Y in so far as X is sweet and Y is bitter ", or, again " in so far as X is smooth and Y is rough ". But, just as none of the orders generated by the particular determinables, colour, taste, texture, can provide the condition for saying that the same X, i.e. one and the same apple, is red and sweet and smooth, and the same Y i.e. one and the same orange, is orange and rough and bitter, so none of these orders can provide sufficient condition for saying " This (one and the same) apple is different from this (one and the same) orange ". If we were limited in our predication to viewing things solely from within a number of particular orders of different determinables we should be

limited to saying, " X is different from Y *in colour* ; X is different from Y *in texture* ; X is different from Y *in taste*. . . ." But we should not, in fact, be able to say that " X " and " Y " in this series of statements stands for one and the same object ; we should not, that is to say, be able to say that one and the same object (*e.g.* This apple), which is smooth and red and sweet is different from one and the same object (*e.g.* This orange), which is rough and orange and bitter. Thus, we have, once again, the problem of difference within unity to explain. But this difference within unity must transcend and at the same time contain the various differences within unities expressed in the logic of positive terms, for these latter unities concern determinables which are all on one level, and the orders they generate fall outside each other. The order we are seeking is one within which all particular differences of colour, taste, etc., fall, and it is one which will enable us to *use* the positive terms like red and smooth and sweet and orange and rough and bitter in marking off one object (This apple) from another (This orange). Here it must be noted that it no longer makes sense to talk about " deciding which differences we shall note ", for we are no longer dealing with an order alongside other possible orders, but with an order which is and must be fundamental, however we may describe it, for it is the order which is to provide the ultimate formal requirements of logical compatibility and which is to make possible the possibility of different orders within which one and the same object may be placed. We can say, therefore, that this order must be a transcendental and immanent order, meaning by this that the place to which an object is assigned in this order, and which is the ground of the notion of " one and the same object ", carries and maintains the object in its identity over and above and in and through the places and partial identities assigned to the object in any one or a number of positive orders. This order can rightly be called absolute in the sense that no object is capable of being viewed as a subject of predication outside of membership of this order. [This last point requires a certain elucidation. My point is that *if* we view a given order, for example, the order of colour, as existing alongside other orders, then this implies that it is logically possible, whether or not, in actual fact it is the case, that one and the same object may belong at once to the order of colour and to an order other than the order of colour. But as I have argued, this possibility cannot be grounded in either of these orders, or in any order which we treat as an order alongside other orders. Hence, to be able to talk of an order in these terms, *i.e.* as a particular order alongside other possible orders, is to

pre-suppose an absolute order within which one and the same object is placed, and it is only *this* absolute order which can therefore provide us with the subject in relation to which we can view the positive terms of these different particular orders as predicates. If, of course, we treat the order of, for example, colour as absolute then it is itself sufficient to provide us with a subject of predication. But we should then be limited in the logical possibilities of predication to those positive terms which serve as indices of the order of colour.] Furthermore, we must describe membership of this order as unconditional in the sense that the notion of "conditions of membership of a given order" presupposes the possibility of viewing a given object as a subject of predication outside of its membership of this given order. We then ask whether or not it possesses the characteristics necessary in order to be viewed under the given order. But, prior to membership of this absolute order, we have no possible subject of predication.

III

The Concept of Being. I do not propose to examine the claim that being is the first intelligible, nor to offer proof of the propositions which thomists use in their elucidation of the concept of being. I shall merely indicate those of them which seem to me to be enlightening to the foregoing considerations :—

(1) For the thomist, cognition is a spiritual activity ; however much the senses may be needed they cannot themselves constitute a cognitive act for this is reflective—a "saying to oneself". On the other hand, our intellect is not intuitive ; and, consequently, all our cognitions involve "mental words", or concepts, by which we say to ourselves that "this is thus and not otherwise". Now, in saying that being is the first intelligible, and that "the understanding of (being) is included in all things whatsoever a man apprehends" what is being claimed is that the viewing of things under the concept of being is the very condition of cognition or intelligible discourse. It is through the concept of being that we are provided with something to talk about, in the sense of a logical subject.

If we ask whether an object can become a subject of predication under any other notion the answer is that being is the first intelligible ; whatever we conceive we conceive in relation to being. To view an object as red or noisy or smooth is still to view it as being red, etc. From our point of view, then, we can say that, according to the thomist, all particular " ordering "

takes place within the order of being : it is the condition of all further ordering.

(2) The concept of being is at once transcendental and immanent. In viewing an object under the concept of being we do not mark it off from anything, or, quite literally, we mark it off from *nothing*. Being transcends the categories. On the other hand, in viewing something under the concept of being we are not viewing it from a mere partial aspect ; everything that is is " of being " by all that it is and in all that it is. We can, if we like, say that it is by virtue of its being that a given object comes under the concept of being, or is a member of the order of being ; but its being is not one characteristic alongside other characteristics, as, for example, the redness of this rose is one characteristic alongside, for example, the other characteristic of its scent. The rose *is* by all that it is and in all that it is. To view an object under the concept of being is to view it in its concrete entirety, and therefore if, to view an object under the concept of being is to mark it off from *nothing*, it is, at the same time, to mark it off from *everything but itself*. " Everything is what it is and not another thing ". Therefore, to view an object under the concept of being is necessarily to place it, even if we cannot give *positive* identification marks of this place, in an order which is transcendental and from which nothing can escape. To say of a given object that " it is not of this (transcendental) order " is, indeed, to employ a term familiar in the writings of Ewing's opponents, " self-stultifying ", since at one and the same time we treat an object as a possible subject of predication and deny that it is a possible subject of predication. Here, at this level, no sense can be given to the idea that we ourselves choose to view things from within this order. The order is given in the sense that the concept of being is the very condition of intelligible discourse.

(3) No object can escape from the order of being, but within this order each object has its own unique place which is the mark of its self-identity and its distinction from all other objects. I have said earlier in this examination that our effort to know the real is necessarily an effort to put or find order in what is given to us in experience. What is distinctive in the thomist interpretation of this cognitive effort is that, for the thomist, what is always being sought is the concrete existent in its place within the absolute, unconditional order of being. But for the human mind this effort is necessarily carried out at the logical level of " composition and division ", *i.e.* of judgements employing abstract concepts, by reason of the fact that we have no direct intellectual vision or intuition of the concrete existent in its very

being. It is true that, in the judgement of existence or being,
we know what is " most intimate " [1] in the given object, but we
know this only in an imperfect and obscure way by means of a
concept—the concept of being—which is applied to everything
else that exists or has being. If, therefore, we were content to
remain at this transcendental level of ordering the objects of our
experience all we could say of each one of them would be that it
is, that it is identical with itself, and distinct from all other mem-
bers of the order of being. We could indeed say that " it is what
it is and not another thing ", but we could not say what it is, and
in what way it is not another thing. Hence we may say that this
original judgement whereby an object becomes a subject of predic-
ation requires and demands to be completed by other judgements
bearing upon one and the same object which will gradually fill up
the distance between the subject (" This "), and the predicate
(" is "). But none of the predicates of these other judgements
can be viewed as being on a level with the predicate, being, since
the precise function of these other predicates is to determine the
manner of being of the object under consideration, *i.e.* the par-
ticular way in and through which it *is*, and in and through which
it is distinct from other beings. The terms employed, therefore,
cannot be transcendental, applying to all subjects of predication
as such, for they are the terms by means of which one subject of
predication is to be identified by being marked off from other
subjects of predication. We may say, therefore, that according
to this theory there is need for particular non-transcendental
orders, in and through which the members of the absolute and
unconditional order of being may be positively identified and
marked off from each other. The human mind or intellect, ac-
cording to St. Thomas, " has to gather knowledge from individual
things by way of the senses " .[2] These non-transcendental
orders, therefore, will be based on the experimental data in and
through which we first become acquainted with existing objects.

(4) We must note, however, that the finding or putting of
order in the objects which we experience always involves the
conceptualisation of the sensible data, or, to put this in a way
more fitting to Ewing's opponents, involves the formalisation of
the words which refer to sense-data. Now I do not suggest that
" conceptualisation " means to the thomist the same as does
" formalisation " to the radical empiricist. But I wish to hold
these two philosophical theories as close together as possible in

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[1] " Esse est illud quod est magis intimum cuilibet ", *Summa Theologica*, I,
Q 8, a. 1.

[2] *Summa Theologica, I*, Q. 76, a. 5.

order to show what it is that is required at the *logical* level if we are to employ orders based on sensible data in the work of identifying and distinguishing the objects of our experience. In this connexion the significance of the theory that being is the first intelligible and that everything that the mind conceives it conceives as " of being " is, briefly, as follows : Suppose that we formulate two judgements, (*a*) This is, and (*b*) This is red. The purpose of the second judgement is to act as a complement to the first in that it, (*b*), belongs amongst those judgements which we employ in order to understand more closely in what way the object referred to by " This " may be said to be. Now, it is clear that the concept of red is distinct from the concept of being, although it must be said to imply it since " being is the first intelligible ", and hence we have, in these two judgements, two distinct predicates attributed to one and the same subject. It is clear therefore that what these concepts refer to cannot be viewed as characterising the object in exactly the same way. Nevertheless, in each case, if the judgement is true, we must describe the relation between the subject and predicate as one of identity. There must therefore be a certain difference in the ground of this identity. In each case the predicate is attributed to the subject, and to the *same* subject, and there must be identity between the subject and predicate, but it is clear that this identity cannot concern the subject in exactly the same way in both cases, and that we must therefore be able to view the subject from more than one standpoint. Now, the distinctive mark of the identity between the predicate, being, and its subject is, as has already been noted, that it is an identity with the subject in the latter's concrete entirety. The subject, " This " *is*, or has being, by all that it is, and in all that it is. We can say that when we view an object from the stand-point of being we view it from an absolute standpoint. In all other cases, however, when we employ concepts which are distinct from the concept of being, in order to increase our knowledge of a given object, the identity between the predicate and the subject concerns the latter only as viewed from a relative standpoint, *e.g.*, " The subject " This " which *is*, is, from the standpoint of colour, red." This does not mean that only part of the given object is red, but that it is red as considered under a certain aspect.

It will be seen from this account that the fundamental difference between the standpoint of being, and the standpoint of these other concepts which are distinct from being is that, while we must resort to these other standpoints in order to make up for the imperfections of our original knowledge of an object

("This is"), we can never rejoin the given object in its concrete entirety through these concepts. The knowledge we gain through the latter is and must remain abstract, for, while it concerns the given object, which is realised and preserved as one and the same subject in the judgement of being—hence the fundamental importance of this judgement—it concerns this object viewed abstractly, from a certain point of view, or from a number of points of view, which can never add up to the absolute point of view. This can only be supplied through the predicate, being, which alone can present the concrete existent as a subject of predication.

In conclusion, therefore, I suggest that the possibility of viewing an object from within different orders presupposes, (1) the possibility of recognising the object as a member of an absolute unconditional order to which it belongs in its concrete existence, and (2) the possibility of viewing this object from different aspects, which can be recognised as such, and the identities involved in predication within these aspects kept distinct from the all embracing identity involved in the absolute unconditional order.   If we have no means of distinguishing between the identities within the relative conditional orders and the identity within the absolute unconditional order then it seems that inevitably we shall erect these relative orders into absolute orders, and we shall no longer be able to explain how it is that concepts or terms belonging to these orders, each of which is now not simply absolute in its own light, but absolutely absolute, can be applied to one and the same subject. Indeed, each of these concepts or terms will necessarily be viewed as representing a value of being with which the given object is identified absolutely, and the *unity* of the given object, which is reflected in the notion of "one and the same subject", will be reduced to a mere togetherness or collection of absolute terms.

VI.—ORDER AND DISORDER IN TIME

By K. W. RANKIN

How far is the accepted analysis of most kinds of serial order applicable to temporal order in particular ? So far as I know this question has never been investigated in any detail. In what follows I shall review two commonly held theories about temporal order, and then outline a third theory. This third theory, I shall try to show, has none of the defects of the other two, and has the added merit of applying with very little modification the general analysis of serial order to temporal order in particular. I shall conclude by showing how it can explain quite simply the disorder in time which the phenomena of precognition may suggest.

I

The following account of serial order in general is given by Russell in his *Introduction to Mathematical Philosophy*. According to him (*op. cit.* pp. 30-31)

> we must not look for the definition of order in the nature of the set of terms to be ordered, since the set of terms has many orders. The order lies not in the class of terms, but in a relation among the members of the class, in respect of which some appear as earlier and some later. The fact that a class may have many orders is due to the fact that there can be many relations holding among the members of one single class.

A set composed of the people in a room may for instance be taken in order of age or in order of height or in some other order. Here the relations which determine the order are ' older than ', or ' taller than ', and it would be a remarkable coincidence if everybody occupied the same position in order of age as he or she occupies in order of height. For this reason Russell has supposed the order does not depend upon the class of terms ordered.

We may now ask the question ' What properties must a relation have in order to give rise to an order ? ' Russell lists three properties as necessary and sufficient. The relation must be asymmetrical, transitive, and connected. In place of our first example of forming a series let us let us turn to the serialisation of a set of events, *e.g.* a number of flashes of coloured light. If we stipulate that no two members of the set are the same in intensity,

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saturation, or duration we can serialise that set in terms of the relation ' intenser than ', ' more saturated than ' and ' longer than '. These three relations are obviously asymmetrical and intransitive ; and our stipulation ensures that, in the instance of this set at least, they are connected. We may note further that here as well the serial order derived in this way may vary according to the relation we select as ordering principle.

I can now state my main problem with greater precision. The flashes of light are all events and as such have a temporal order in addition to the other three orders. Now is this temporal order also dependent upon a relation ? If so, upon what relation ? And could this relation be just one among a number of relations such as those I have specified ? Or is it in some way more fundamental than these ? Is Russell's statement that ' order lies not in the class of terms but in a relation among the members of the class ' true in any way of temporal order ? I shall argue that it is true in some respects but not in others. Temporal order depends upon a relation between members of a class, but this relation is basic and not accidental to the various members of that class as members of the same class. Temporal order is in fact unique among orders in a *number* of respects and I shall go on to determine these respects.
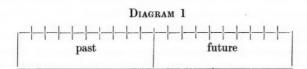
To begin with, the temporal order of a set of events seems inseparable from that set taken as a set of particular actual individual events. I do not deny that the same set of events may have very different orders according to the relation that one selects. I merely aver that if the temporal order depends upon a relation, that relation, and that relation alone, must be basic to the existence of the set or sets of things which actually happen. Here, I suppose, I am struggling towards one facet of the truism that temporal order is the order in which events *come to be* or occur, although to link this order with a relation is more than a truism. This relation, one must emphasise, should be basic to the *existence* of the members of the set as *individuals*, and not to the properties which these individuals possess.

The set of light flashes may serve to establish this point. It might conceivably have an exact replica in another set such that both sets correspond exactly in the intensity, saturation, and duration of their members. Now no matter how complete the correspondence between members of the two sets may be in these and *any other characteristics*, it remains perfectly possible for the temporal order of the members in one set to be quite different in that set from the temporal order of their exact replicas in the other set. Again we might take a set of events

which were exactly similar in all their characteristics.   If the temporal order depended upon relations in respect of any of the characteristics which they can have in common with each other all the members of this set would accordingly be simultaneous with each other.   But there seems to be no logical necessity for instances of any common characteristic or set of characteristics to be simultaneous.   Another indication of the same fact is that when we are given that a number of events have occurred, we have to rely upon observation to determine the order in which they have occurred.   We can never derive the order solely from a complete knowledge of their characteristics, independently of inductively established causal laws.   Accordingly, temporal order is basic to the existence of events as individually distinct from each other rather than as instances of certain characteristics.

Here, then, we have one important difference between temporal order and other kinds.   There is, however, a further peculiarity which may seem even more distinctive : temporal order seems bound up with the distinction between past, present and future in a manner which has no parallel in other kinds of order.   The temporal series appears to be divided at an instant which we call ' the present ' in such a way that every event antecedent to that instant falls in what we call ' the past ' and every subsequent event in what we call ' the future '.

DIAGRAM 1

These distinctions are also indicated in other ways, *e.g.* by tense inflections.   Thus at the present in 1956 we say ' Mr. Marshall went to London before Tengku Abdul Rahman ' whereas a year earlier anyone privy to the councils of these two ministers would have had to say ' Mr. Marshall will go to London before Tengku Abdul Rahman '.   Inflections of this kind are only required when we discuss the relations of before and after between events.   They are quite irrelevant to the serial order of things which have no location in time.   For instance it would be pointless though not meaningless to say ' 1 was before 2 ' or ' 1 will be before 2 ' instead of ' 1 is before 2 '.   In dealing with the order of nontemporal entities we use what I shall from now on call the timeless present tense of the appropriate verb.

But we haven't yet drawn attention to the most distinctive

feature of tense-distinctions.   What I called 'the present' in 1955 is not the same moment as the moment I call ' the present ' now. Likewise what I called ' past ' and ' future ' then, coincides only partially with what I call ' past ' and ' future ' now.   This fact has given rise to a lot of metaphorical and misleading talk about time flowing or the present moving out of the future into the past. These metaphors treat time or the present as if it were itself a process in time or something which takes time to do something : and it is for this reason that they are misleading.   Nevertheless they point to something which seems to require some sort of analysis.   There is one thing in particular which deserves explanation.   The present is, as we have seen, itself an instant in the temporal sequence to which all the other instants are referred when we discuss their interrelationships.   But this reference point is not a matter of arbitrary selection.   We seem in some way committed to refer all other instants to one particular instant rather than any other.

These then are the two distinctive features of temporal sequence.   There are, however, at least two further peculiarities of temporal sequence as we experience it which may seem more than accidental to its nature.   The first of these consists in the fact that we regard the future and not the past as being dependent upon our actions.   The second is that apart from the unreliable phenomenon of precognition the future is unknown to us in a way in which the past is not.   We can remember the past : but there isn't anything like this which we can do to the future.

To sum up, then, the temporal series seems to have four characteristics which distinguish it from any other series.   These are

(a) The relative order between the instants of the temporal series is referred to one member of the series which we call the present and this reference point though variable doesn't seem to be chosen at will.

(b) The temporal order is basic to the existence of events as individual entities.

(c) The future appears dependent upon our actions in a peculiar way in which the past is not.

(d) The future appears to be open, or at least unknown, in a way in which the past is not.

With this list I conclude the investigation of my problem.   The problem is to find a relation holding between the members of the temporal series which is such that we can derive an order which shares a significant number of these four characteristics and which, accordingly, can fairly be represented as the principle of temporal

order. But before propounding a relation of this kind let us look at two theories which view the problem rather differently. They both attempt to account for certain of the four characteristics while at the same time making no attempt whatever to derive temporal order from a more fundamental relation. Consistently with these two theories it would be possible to hold that temporal order, unlike the other kinds of order, is far too fundamental a type of relation to admit of further analysis or derivation from an even more fundamental principle. Both theories, however, give an analysis of the distinction between past, present and future and the use of tense inflections. It will be instructive to consider in what way, if any, their analysis is satisfactory before attempting a more ambitious explanation.

Both theories begin on the assumption that fundamentally the temporal order can be represented in uninflected language. We can in other words use the timeless present as in ' Mr. Marshall goes to London before Tengku Abdul Rahman ' instead of either ' Mr. Marshall went to London before Tengku Abdul Rahman ' or ' Mr. Marshall will go to London before Tengku Abdul Rahman '. What, then, do we do when we use temporal inflections, and why do we use them ? Here the two theories diverge, although according to both the function of the tense inflections is to refer one series of events to a concurrent series of a special kind.

The first theory I shall call the token-reflexive analysis.[1] According to this theory the tense inflection indicates the relation of the event which the verb describes to the utterance which one is making. One relates, in other words, the series of events which one describes to the series of events consisting of one's utterances. Thus ' A was before B ' is supposed to mean ' A is (timeless present) before B and A is (timeless present) before this particular utterance '. Again ' A will be before B ' is supposed to mean ' A is (timeless present) before B and A is (timeless present) after this particular utterance '. In this way tense inflections have been altogether eliminated by referring events to utterances.

DIAGRAM 2

	1	2	3	4	5
Heterogeneous series	O	O	O	O	O
Linguistic series	U	U	U	U	U

[1] *Cf.* H. Reichenbach, *Elements of Logic*, ch. 7, § 51 ; and, for a more refined version, L. Jonathan Cohen, *Tense Usage and Propositions* (Analysis, vol, II. No. 4, 1951).

The second theory is rather similar [1]. I shall call it the epistemological analysis. According to this theory the order of events is referred not to a concurrent series of utterances but to a concurrent series of acts of knowledge or cognition. Thus ' A was before B ' is supposed to mean ' A is (timeless present) before B and A is (timeless present) before this particular act of awareness '. Likewise ' A will be before B ' is supposed to mean ' A is (timeless present) before B and A is (timeless present) after this particular act of awareness '. Once more the uninflected way of describing temporal order is regarded as basic.

DIAGRAM 3

Heterogeneous series (O)

Cognitive series (>)

Let us now compare the relative and absolute merits of these two theories by seeing whether they account for any of the characteristics of temporal series previously listed. Both account for a distinction which might be labelled ' past ', ' present ' and ' future '. Both, likewise, seem to explain why the labelling of one instant as ' the present ' could not be a matter of whim or arbitrary selection. In making an utterance one commits oneself by a logical necessity to accepting the moment of one's utterance as the present according to the token-reflexive analysis. I cannot now consistently say ' The present is 1066 ' since in making the utterance I indicate that the present is the moment of my utterance. Similarly, on the epistemological analysis my act of awareness commits me to take the moment of awareness and no other as the present.

On the other hand the token-reflexive theory accounts neither for the fact that the future rather than the past is dependent upon my actions nor for the fact that I am ignorant of the future in a way in which I am not ignorant of the past. The epistemological theory accounts for them slightly, but only slightly, more successfully.

It will be noticed that both analyses treat the temporal series as absolutely complete. All the events that ever have occurred,

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[1] *Cf.* A. J. Ayer, " Statements about the Past " (Proc., Arist. Soc., 1951-52).

are occurring now, or ever will occur are, in a timeless sense, completely there. This would apply, of course, equally to the series of events consisting of my utterances or of my acts of awareness. Now the token-reflexive analysis seems to ignore that my utterances are at least sometimes the result of intelligent activity and something under my control. For instance, if it is now my opinion that it will rain tomorrow, I have a choice either of saying now ' It will rain tomorrow ' or of waiting until it has rained in order to say ' It has rained ' or, for that matter, of doing neither or both of these things. On the other hand, if it has rained and I wish to refer to the occurrence, I have no comparable choice. I can only say now ' It has rained '. There is no comparable process whereby I can work round, as it were, to the other side of the event in order truly to say ' It will rain '. I have, it may be observed, avoided the simpler mode of putting this point. With less circumlocution one might say that one cannot *choose to wait* on the past in order to say ' It will rain '. But though this impossibility may very well be of a purely logical sort one might wrongly identify it with a logical impossibility of a rather insignificant kind. One might say : ' Of course we cannot wait on the past ; for *by definition* we can only wait on the future. And for the same reason, of course one cannot choose now that something should have happened in the past.' But the impossibility of waiting on the past is something more than the impossibility of, say, seeing a sound. There is a form of sound-apprehension comparable to sight-apprehension. On the other hand there is no process with respect to the past which is obviously comparable to waiting—and a similar conclusion holds for choosing.

Now this fact may not seem to upset the token-reflexive analysis, but at least one can say that the token-reflexive analysis does not account for it. Accordingly our attitude towards that analysis must be determined by whether we think such facts about waiting and choosing are directly connected with the distinction between future and past.

The epistemological analysis on the other hand can effect some sort of connection. In accordance with it we can suppose that memory works backwards in time instead of forwards *just as a matter of psychological fact,* and that there is nothing in the objective world apart from us to prevent the order from being reversed, so that we would ' remember ' what is about to occur rather than what has occurred. There would be nothing in the objective order to prevent the occurrence of a race of men who knew more in the womb than in their days of physical maturity,

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and who as time advanced progressively unlearnt everything they once knew without acquiring any further knowledge. As it is, however, instead of ' remembering ' the future, we have to make do with rather unreliable predictions on the evidence of the past. We never, or at least seldom, retrodict the past on the evidence of the future.

The epistemological analysis has accordingly an advantage over the token-reflexive analysis of being able to explain three of the characteristics noted about temporal order with greater economy in terms of one hypothesis. It explains, firstly, why the present isn't an arbitrarily chosen date, secondly, why the future rather than the past seems to depend on our action, and thirdly, why the future seems open. This it does simply by accepting as a psychological fact about awareness that it is open to what precedes (timeless present) it in time and closed to what succeeds it. Diagram 3 shows this by using the sign ' > ' for awareness. Thus any act of awareness has as its object physical events which immediately precede (timeless present) it and also acts of awareness which precede (timeless present) it. It excludes from its content all succeeding physical events and acts of awareness. Nevertheless a definite and complete set of subsequent events is (timeless present) there.

I think this type of theory still cannot give an adequate account of voluntary action, but to show this here would involve disproportionate length. The theory, however, does make a gesture towards an account of voluntary action, and choice in particular, by requiring that the future should be in some sense open. It is the openness of the future which makes it, rather than the past, the concern of choice. For the same reason it makes a gesture towards explaining why it is for the future and not the past that we wait. Two points that tell against this theory can, however, be made quite briefly and they speed us along the road to the third theory I shall consider. According to the epistemological theory, since we are ignorant of the future there is no way of determining whether our ignorance is really no more than a psychological peculiarity of human beings. It might just as well be due to some objective characteristic of the future apart from us, *i.e.* to the fact that the future really is indeterminate. The second point is that the epistemological analysis, and likewise the token-reflexive analysis, give no explanation of why temporal order unlike other orders seems basic to the existence of the events ordered.

The stage is now set for the entry of the third theory of time, which explains all four temporal characteristics in terms of the

same hypothesis. This theory can be taken as an interpretation of the truism that the order of events is the order in which they come into existence or come to be. Strictly speaking it does not deny that the temporal series is (timeless present) completely there. More accurately it denies that the timeless ' is ' can properly be used for referring to events. The relation from which temporal order is derived is that of one-many necessary connection.
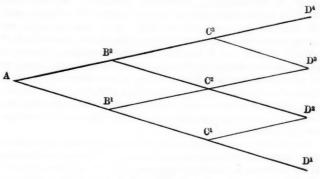
DIAGRAM 4

(In a less compact diagram than 4 all lines leading from A to D would diverge and none would converge. To begin with I shall attend to the divergence alone and shall ignore the convergence. In the sequel when offering an interpretation of precognitive phenomena I shall show that this convergence towards D has a more significant property than compactness).

This one-many relationship is such that, in terms of the diagram, A entails and is entailed by the complete and exhaustive disjunction B^1 or B^2, B^1 entails and is entailed by the complete and exhaustive disjunction C^2 or C^3, and so on. To say B^1 or B^2 is a complete and exhaustive disjunction is here taken to mean that B^1 and B^2 are possible, that only B^1 and B^2 are possible and that in addition they are incompatible. Now it will be observed that where we take A as having determinate existence the complete and exhaustive disjunction B must have indeterminate existence—for indeterminate existence I take by definition to consist of equal possibilities and the incompatible elements of B are both equally possible. For similar reasons the triplet C and the quadruplet D must have an indeterminate existence. There must in fact be three different orders of indeterminate existence

depending upon whether the group is the B, C or D group. One can represent these relations differently by saying that where A has existence, any basic element of B has merely possibility of existence of the first order, any basic element of C has merely possibility of existence of the second order and any basic element of D has merely possibility of existence of the third order. Now let us turn to the significance of this pattern of relationships.

It will be noticed that between the groups A, B, C and D we have an asymmetrical, transitive, connected relation. The relations can be described as ' having members of a higher order of indeterminacy or mere possibility than '. Accordingly, we can arrange these groups in such an order that A comes first, B second, C third and D fourth. A further significant feature is that this order is basic to the existence of the individual members of these four groups. It is an order of possibility of existence with actual existence serving as a limit of this order. Accordingly, we have constructed a relation from which an order can be derived possessing at least one of the distinctive characteristics of temporal series.

Let us now turn to the other characteristics and first of all to the distinction between past and future. A similar distinction is also called for in the interrelated set which we have constructed. We began by taking A as having actual determinate existence, and the mode of existence of all the other terms was determined by this postulate. But what happens if we take another postulate and confer actual determinate existence upon, say, B^1 instead of A. When we do this the set of interrelated terms immediately becomes divisible into two sets. One set consists of the terms which are entailed by but do not entail B^1 and the other set consists of the terms which both entail and are entailed by B^1. Thus A is entailed by but does not entail B^1. Accordingly, if we postulate that B^1 has determinate actual existence we must likewise suppose that A has determinate existence. Again the two disjunctions of C^1 and C^2, and D^1, D^2 and D^3 both entail and are entailed by B^1. Accordingly, relative to B^1 each of the individual elements of these disjunctions has in varying orders only possibility of existence. This distinction between two distinct sets, one of actual determinate existents and the other of possibilities, seems to correspond closely with the familiar distinction between past and future. But its correspondence to the distinction between past and future extends even further in a number of other respects. For instance, we noted earlier that the division between the past and the future is not an arbitrary one depending upon whim. Now it may be objected that we

constructed two sets from our set of interrelated terms simply by postulating in an entirely arbitrary manner that B^1 had actual determinate existence. But mightn't we with as much justification have postulated that any of the other terms had actual determinate existence ? The answer is ' no '. It is by a logical necessity that we must refer the relations between the serially ordered sets of terms to one of their number and no other. This key term determines the precise constitution of the rest of the set to which it belongs. Thus B^1 rules out the following terms as possibilities of any order of existence, *viz.* C^3 and D^4. If in contrast we were to postulate C^1 as having actual determinate existence we would in addition rule out D^3 as having possibility of existence of any order. In other words the point at which the set of determinate existents divides from the set of indeterminate existents or the set of possibilities depends upon the precise membership of these sets. No other term in either of these sets could serve as the point of division without broadening or narrowing the membership of these sets. Therefore, within any set of terms it is not an arbitrary matter which term functions as the point of division between the two subsets.

Two temporal characteristics out of the above list now remain for consideration. We saw that the future seems dependent upon our actions in a way in which the past is not, and also that it appears open in a way in which the past is not. Our diagram displays a system of relationships which possess similar characteristics. The analogy between the set of disjunctions determined, say, by B^1 and the openness of the future does not require further elaboration. And there is a similar analogy between the set of determinates and the closedness of the past. Likewise the independence of the past and the dependence of the future upon action is analogous to the way that B^1 and B^2 both determine exactly the same set of determinates, *viz.* A, but slightly different sets of disjunctions. B^1 excludes any disjunction of which C^3 and D^4 are elements, and B^2 excludes any disjunction of which C^1 and D^1 are elements.

We have, accordingly, constructed a relationship from which we have derived an order which exhibits all four temporal characteristics listed above. It is reasonable, then, to interpret the terms of the relationship as being events, things which occur, come to be, or take place, *when viewed in this relationship, and* the resulting order as being the temporal order or order of occurring.[1]

[1] *Cf.* D. Y. Despandé, *Professor Ayer on the Past* (MIND, January 1956, pp. 86 ff.).

Before concluding the first half of my programme I wish, however, to comment upon one basic assumption and one salient consequence. The basic assumption is the notion of necessary connection. This I just take as a primitive notion, as a notion even more primitive than that of temporal order ; and I make no attempt to define it further except in so far as a notion is defined in terms of its logical implications. As for the salient consequence, it may on first inspection have the appearance of paradox.

Of two equal possibilities for the future what at a later moment distinguishes the one as having and the other as not having occurred ? According to my analysis this question can only be answered up to a point. Beyond that point it is meaningless.

There is nothing in the relationship between the event which has occurred and its antecedents to distinguish it from the event which has not occurred—for otherwise the event could not be related to its antecedent as later to earlier event. The sole distinction between what earlier were two rival possibilities is that each determines a set of further possibilities for the future which is incompatible with the set determined by the other. Therefore what distinguishes one event as having occurred from its erstwhile rival possibility is that the set which it determines as possibilities is the future whereas the set which the other determines as possibilities is not the future. Thus what distinguishes Singapore's failure to achieve independence from Britain in May 1956 as something which has occurred is the fact that after May 1956 Britain can constitutionally still use her troops in Singapore as she pleases. This sort of answer may seem only to postpone the issue, for one can proceed to ask what distinguishes this particular set as really being possibilities for the future from that set as no longer being possibilities for the future. To this question the only answer is either circular or one which raises the same question at a later stage. It is circular to say ' These are the possibilities for the future because they are determined as such by what has just occurred '. On the other hand it postpones the issue to say ' These are the possibilities for the future because they determine the possibilities of the yet remoter future '. This sort of inconclusiveness can, however, be interpreted as reflecting our actual thinking about time. It would explain our tendency to think there must always be a future on the principle that unless there always is a future there could be no distinction between what has occurred and what has not.

We can now proceed to the second half of my programme and deal with temporal disorder.

II

If the temporal order of a set of events depends upon a certain relationship it follows that where this relationship is defective in asymmetry, or transitivity, or connexity, temporal order will be replaced by temporal disorder. All three of these three properties we saw, must characterise any relation which orders perfectly. Now one of the ways in which time may be disorderly is represented by the compactness of Diagram 4. This has produced a failure in the asymmetry of the relation between certain of the terms. B^2 is the referent of a one-many relation with C^2 and C^3, but likewise C^2 is the referent of a one-many relation with B^1 and B^2. Perhaps the symmetry is brought out more clearly if we say that not only is B^2 the indispensable and sufficient condition of a disjunction between C^2 and something else, but that C^2 is likewise an indispensable and sufficient condition of a disjunction between B^2 and something else. If we express the symmetry in this way we should notice, however, that this sort of symmetry in the relation between B^2 and C^2 is reinforced, by a similar sort of symmetry between B^1 and C^2. Not only is B^1 the indispensable and sufficient condition of a disjunction between C^2 and something else, but also C^2 is an indispensable and sufficient condition of a disjunction between B^1 and something else. We can see, of course, that the bare distinction between symmetry and asymmetry isn't really sufficiently sensitive to all the possible ways in which relations apart from the one-one type may differ from their converses, but there is no need to enter into these niceties here.

Now if temporal order is derivable from the relation of one-many necessary connexion, there might be certain temporal anomalies brought about by a defective asymmetry of the kind which Diagram 4 represents. If we interpret C^2 as an event, it would in certain respects be successor to A and to either B^1 and B^2. In so far as it is a relatum of a one-many relationship of which either B^1 or B^2 is the referent, it succeeds A and the latter two. But in so far as it is the referent of a one-many relation to B^1 and B^2 it is temporally prior to either of these, and, furthermore, it must compete with A for the position of immediate antecedence to B^1 or B^2. Of course, the respects in which C^2 succeeds B^1 or B^2 preponderate over the respects in which it comes prior to them. C^2 relative to B^1 and B^2 is disjunctively related to C^1 and C^3 respectively. Accordingly the respects in which it succeeds either B^1 or B^2 are twice as strong as the respects in which it precedes them.

It is possible that these sources of disorder are of relevance to the phenomena or alleged phenomona of precognition. Let us take ' precognitive ' experience at its face value and examine more fully its implications. One fact of importance is that so far as the evidence goes we do not precognise the whole of our future. Why is this ? It might seem to have the same sort of explanation as the fact that we do not remember the whole of our past. This seems to be due to psychological or physiological reasons rather than an indication of any incompleteness in our past. We may remember, forget, and remember again the same event several times over. This suggests that there actually are events in the past even at the points where there are gaps in our memories. On the other hand, we precognise very much less of the future than we remember of the past. Of course we might conceivably precognise, ' forget ', and precognise again the same event in the future. But the important point is that if we do this at all, we do it very much less than we do the comparable things to the past. It must be admitted that we probably have many more precognitions than we recognise as such. Many may take place in dreams which we later forget. But it remains plausible that the apparent infrequency of our precognitions is real and at least partly due to an incompleteness in the future rather than to a gap in our awareness. Much of the future may not be determinate enough to be precognised.

The application of Diagram 4 to the explanation of precognition should now be apparent. It explains why the same event, *e.g.* one represented by C^2 may seem later in time than another event, *e.g.* B^1 or B^2, and yet to compete for the same position of antecedence before B^1 or B^2 as is occupied by A. In so far then as this sort of situation is the sort of situation we describe as ' precognitive ', the analysis of precognition given here is adequate. It also accounts for the relative patchiness of precognition since in our diagram D^2 and D^2 are the only other terms in a similar position to C^2. Other diagrams are possible, of course, in which the occurrence of such anomalies are less frequent.

There are, however, an indefinite number of aspects of precognitive experience which in this explanation have been or may seem to have been left out of account. No doubt the explanation of some of these would require auxiliary hypotheses. The whole matter invites a more careful weighing than I can conveniently here perform. My provisional estimate is that I have attended to the kernel of precognitive experience.

Diagram 4 has one further implication for precognitive experience which I can now make explicit. On this interpretation the

event precognised will not necessarily occur. In the diagram the possibility of C^1 and C^3 relative to A is not removed. A does not necessitate C^2 as against C^1 and C^3, although on the other hand, C^2 does necessitate A. If then a necessary condition of genuine precognitive experience is that the event cognised must necessarily occur then Diagram 4 fails to account for precognitive experience. Now it may seem that the actual occurrence of an event is our principal reason for saying that an event is ' precognised '. But the strict conclusion is merely that the use of a veridical term like ' precognised ' may be question-begging. It may be that all one needs to explain is why an experience which we have actually had seems *in a manner* to have occupied a previous position in time. There may be no need to infer from the fact that it has occupied in this manner a previous position in time that its subsequent occurrence is necessary. All we may need to suppose is that, whether it occurs or not, it has in certain limited respects an indeterminate temporal position ; for, of course, events which may possibly occur have a relative temporal order as well as events that do occur. On this interpretation, then, precognition is not the direct experience at an earlier moment of an event that is definitely located at a later moment. It is, on the contrary, an indefinitely located experience of an event : and this experience need not be one which actually has taken place or will actually take place—it may be one that could have taken place or could be about to take place.

On my interpretation there is, nevertheless, something approximating towards necessity about a ' precognised ' event. From Diagram 4 one can see at a glance that C^2, for instance, is more probable relative to A than either C^1 or C^3. In fact it is twice as probable as either C^1 or C^3 taken individually. Accordingly, I can maintain that ' precognitive ' experience gives some indication of what *will* happen as against what *may* happen later. In a modified way, then, the use of the adjective ' precognitive ' has a justification, though a ' precognitive ' experience is no *certain* guarantee for the later occurrence of an event. I shall refrain, here, from investigating alternative diagrams which by representing a slightly different nexus of relation might superficially appear to endow precognitive experience with greater predictive power.

Both halves of my programme are now complete, but before concluding I wish to consider briefly the relevance of my analysis to the law of entropy. According to this law later states of the universe are characterised by a higher degree of randomness than earlier. Accordingly, entropy or increase in randomness can be

used as a sort of temporal signpost or physical criterion of temporal order. With a certain amount of diffidence I suggest that this increase in randomness is precisely what Diagram 4 represesents.   In that case entropy is not merely a criterion for temporal order but a logical consequence as well. My diffidence here is caused by an alternative formulation of the law. According to this, energy in the universe is becoming progressively disorganised, and I find the concept of disorganised energy rather hard to translate into concrete terms.

One final comment. In its details my interpretation of Diagram 4 in terms of temporal order is in many places vague or apparently arbitrary.   In some instances this has been deliberate with the intention of cutting down the number of related issues.

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VII.—FINITE AND INFINITE

By Thomas McPherson

It is not so widely thought today as it once was that the existence of God can be proved by rational argument, but this belief does survive in at least one important place. Thomist writers maintain that the traditional Cosmological Argument, properly understood, is a valid proof of the existence of God. For the modern Thomist the point of the Cosmological Argument lies in its high-lighting of the work of God as ' upholder ' or sustainer of the universe. Recent interpretations of the Cosmological Argument have developed this side of it strikingly. The line of thought is somewhat as follows :

If you contemplate the world you see it to be ' finite ' and ' contingent ' ; you see that things are, as Dr. E. L. Mascall says, not able to ' account for their own occurrence '.[1] As long as we simply ' take the world for granted ' we shall not see God, but if we become conscious of the finiteness of the world we may come to see that there must be an infinite God. If we become conscious of the contingency of the things in the world we may become conscious that there must be a Necessary Being Who is constantly upholding it. So that the way to see that God exists is first to consider the fact that the world is finite—where ' finite ' means ' not able to account for its own occurrence ' ; when a man has seen that the world is finite he may come to see that there must be a God.

Now it seems to me that the trouble with this as an argument for the existence of God is that it is either not an argument at all but a statement of the obvious, or if it is an argument it fails because it leaves out a most important step.

To say that if we see the finiteness of the world we shall see that there must be a God is from one point of view not an argument but an obviously true statement. For consider what is meant by ' seeing the finiteness of the world '. ' Finite ' only has meaning in relation to ' infinite '. You cannot see something to be finite unless you are also somehow seeing something else to be infinite. We are invited to observe the finiteness of the world as if this were the first step in an argument for the existence of God. But to have seen the finiteness of the world is not to have taken the first

[1] E. L. Mascall, *Existence and Analogy* (London, 1949), p. 85.

step in such an argument ; it is to have gone the whole way. This is not an argument for the existence of God but, in effect, a statement that God exists. To say ' the world is finite '— *really understanding what you are saying*—is to say ' there is an Infinite Being ' ; ' the world is finite ' makes no sense otherwise.

To put the objection more accurately : We are invited to make a kind of comparison between the ' observed ' finite world and the infinite God lying behind it. It is clear that we may compare the finite world either with the mere *idea* of an Infinite Being or with an actual Infinite Being which is ' seen ' to *exist*. In the first case, we will soon be involved in the well-known difficulties of the Ontological Argument, for we have, in order to produce God, still to get from the idea to the actual existence of the object of that idea. In the second case, the Argument is quite clearly no argument for, but simply a rather disguised statement of, the existence of God.

The fault may seem to lie in the use of the word ' finite ', which is a philosopher's word, and, as I have said, is so used by philosophers that it is only to be understood in relation to ' infinite '—as ' infinite ' is only to be understood in relation to it. But if we were to use other terms like ' seeing that things are incomplete ', or, to quote Dr. Mascall's phrase once more, ' seeing that things can't account for their own occurrence ', it would amount to the same. To see things in this way is to see them in contrast to the sort of existence God has. Unless you already believe in God you just do not see things this way ; and if you do already believe in God you have no need of arguments. When we consider the Argument, as we now shall, *as* an argument, and see the step that it leaves out, this point will emerge more clearly.

If the Cosmological Argument is an argument it leaves out a most important step. Those who offer the Argument are saying in effect : Look at the finiteness of the world ; and lo ! you will see God in it, if only you truly contemplate what you see. But the trouble is that it is only believers in God who do see the world to be finite—in the sense that the Thomists mean, anyway. The non-believer, when he looks at the world, sees only the world ; to him it may seem perfectly self-sufficient. If he saw not just the world but the finiteness of the world he would not be a non-believer but would be already a believer.

The Argument is made to run like this : look at the finiteness of the world ; then take the next step to seeing that there must be a God. This, as we saw, is no argument at all, for there is no step here ; seeing the world as finite is not really anything

different from seeing that God exists. The argument comes to something like : ' If you see that there is an Infinite Being you see that there is an Infinite Being '. There is nothing wrong with this, for it is a truism, but one should not dress it up as an argument when it obviously is not.

The real step that must be taken comes earlier and is quite left out. The argument ought to run : Look at the world ; now take the step of seeing the world to be finite ; now you have come to see that there is an Infinite Being.

Here, then, is the real step. But how is it to be made ? Only by a change in a man's whole outlook. It seems to me that the value of the Cosmological Argument lies not in its efficacy as a proof of the existence of God but in its bringing out the point that the believer just is the man who ' sees the world to be finite ', or 'sees the things of the world to be not capable of accounting for their own occurrence '. But it will not do to dismiss, perhaps a little patronisingly, those who persist in ' limiting their gaze to the phenomenal surface of reality '.[1] If a man does not see the world to be finite he cannot help it. He is not to be blamed for this. If he did see the world to be finite he would be a believer, and we know that not all men are believers. His whole outlook on things must change before he will see the finiteness of the world ; but once he has seen it, he is not merely on the first step of the journey to belief in God ; he is a believer already ; and no one in his senses thinks a man is easily brought to that stage. The step the Argument leaves out is the thing that matters, but a man is not easily made to take that step by being argued into it. One should be careful before complaining because men are wisely cautious about saying something they do not feel ; for why should they admit the world to be finite if they simply do not see it that way ? This is not, indeed, to deny that sometimes there is a kind of stubbornness about a man's refusal to ' change his outlook '—sometimes, perhaps, even a kind of stupidity ; but this is certainly not true of all men.

Some, at least, of what I have been saying may strike the reader as hardly needing to be said. Surely all this is not news to the modern Thomist ? We cannot do better than take Dr. Mascall as their spokesman once again ; for his book *Existence and Analogy* seems to me, even where I disagree with it, to be a remarkably acute discussion of the themes it treats. Now, does not Dr. Mascall himself say that the Cosmolog cal

[1] Mascall, *op. cit.* p. 90.

Argument is not strictly an argument ?   He writes: 'The
primary requirement if we are to pass from the recognition of
the finite to the affirmation of the infinite is not that we shall
be skilled in the manipulation of Aristotelian logic but that we
shall grasp in its ontological reality the act by which finite
existents exist.' [1] But consider the significance of this. What
the Thomist position sometimes seems to come to is that know-
ledge of the existence of God is seen to be on a complete
examination not something got as a result of rational *argument*
so much as something that depends on our grasping, as it were
immediately, by a kind of 'apprehension', a truth about the
nature of the world.   Yet at the same time, it seems to me,
Thomists want to stress that in some sense an element of
'inference' must always come into our knowledge of God;
that it can never be a completely 'direct', 'immediated'
knowledge; that it is, in fact, in some sense, based on a process
of reasoning, of argument. It seems to me that there may be
an unresolved conflict in their position.

If the world is finite, God is infinite.   Perhaps some comments
on ' infinite ' would not be out of place here.

'Infinite' is a word almost without content when without
context. And when in context it is often just a resounding,
comforting word, giving little except backing to what is said by
the words around it. We apply the words ' finite ' and ' infinite '
to different concepts : time, space, number, wisdom, etc. If
we say, ' God is infinitely wise ' we mean something ; just as it
would mean something to say, ' Man's wisdom is finite '. ' In-
finite ' is used, first, to mean ' very great of its kind ', or, secondly
—and this is usvally the sense in question when God is being
discussed—, ' incapable of being measured ', *i.e.* ' not the sort
of thing that it is logically possible to measure '. If we say
of a particular man, ' He is infinitely wise ', we are using
' infinite ' in the first way ; it is only of God that we can
say ' He is infinitely wise ' using ' infinite ' in the second way.
To say ' God is infinitely wise ' is not to say ' God is very wise
indeed '.   It is to say something like : ' God is wise, and His sort
of wisdom is not the sort there can be less or more of '. The same
is true of ' God is all-powerful ', ' God is all-good ' : not ' very
powerful indeed ' or ' very good indeed ', but ' powerful ' and
' good ' in a sort of way that does not admit of degrees ; just as
' infinite in time ' does not mean, in this second use of ' infinite ',

[1] Mascall, *op. cit.* p. 78.

' for a very long time ' but ' timeless, not capable of measurement in any unit of duration '.

Now ' God is infinitely wise, infinitely good, infinitely powerful ' means something. But what does ' God is infinite ' mean ? ' Infinite ', used alone, has little or no meaning. It gets meaning when you say what sphere you are talking about—*e.g.* infinite in respect of size, or duration, or wisdom. Strictly, the sentence ' God is infinite ' means nothing, because we have not specified the sphere we are talking about. In fact, ' God is infinite ' is probably often used as a shorthand form of ' God is infinitely wise, infinitely good, infinitely powerful, etc.', and so it will import meaning from these other terms ; but the fact remains that strictly we seem to be saying something empty in saying only ' God is infinite '.

' Infinite ', indeed, seems redundant when added to a list of God's properties. Descartes's famous description of God, as mediated by Veitch, runs : ' God is a substance infinite, eternal, immutable, independent, all-knowing, all-powerful '. What purpose does ' infinite ' serve here ? It says nothing itself, but perhaps says something when combined with the other words in the description ; though there are certainly some—like ' eternal '—to which it can hardly add much. We may usefully compare with Descartes's description of God that in the *Westminster Larger Catechism*. ' Q 7. *What is God ?* A. God is a Spirit, in and of himself infinite in being, glory, blessedness, and perfection ; all-sufficient, eternal, unchangeable, incomprehensible, everywhere present, almighty, knowing all things, most wise, most holy, most just, most merciful and gracious, long-suffering, and abundant in goodness and truth '. The *Catechism* gives more sense to ' infinite ' than Descartes does.

' *X* is infinite ' calls for the question ' Infinite in respect of what ? '. So to explain ' God is infinite ' one must list the properties of God. There is a difficulty here for the modern Thomist approach to the theistic proofs. Their approach—and it is a valuable one—consists in playing down the listing of God's properties or attributes as a necessary preliminary to proving His existence. They point out that one does not get to *that* God is by way of *what* God is ; that, indeed, the reverse of this is the true order ; hence the name ' existentialism ' as applied by Thomists, who maintain that the existence of God is ' prior to ' the essence, or nature, of God ; what precisely is involved in this we need not go into here.

But—and this is the difficulty—if ' infinite ' is empty unless one specifies the respects in which infinity is meant, and if ' infinite '

and ' finite ' are only to be understood in terms of each other, wh becomes of the pure Thomist existentialist position, and Cosmological Argument which is thought to express it ?   To se the world to be finite is to see it to be finite *in certain respects,* and one must be able to specify those respects.   To have seen the world to be finite is also to have seen that an Infinite Being must exist, in the sense that one has seen that there must be a being who is infinite *in the respects in which the world is finite.* But if this is so, then on this approach it is really through the attributes or properties—the essence or nature—of God that one gets to knowledge of the existence of God.   The modern Thomist existentialists are not as existentialist as they think they are.

One point ought perhaps to be added.   It might be said that the Thomists seem to escape this objection on their own apparently rather different definitions of ' finite ' and ' infinite', where the former means ' not able to account for its own occurrence ' and the latter, presumably, ' able to account for its own occurrence '.   I find the phrase ' not able to account for its own occurrence ' unclear, but I suspect that when unpacked it must mean something like ' not able to account for its own occurrence *because* limited in such and such respects ' ; in other words, I suspect that to the Thomist ' finite ' means in the end what it means to the rest of us ; and if this is so the objection that I have just made to the Thomist claim to be an existentialist philosophy stands.

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VI.—DISCUSSIONS

MR. STRAWSON ON REFERRING

MR. P. F. STRAWSON published in MIND of 1950 an article called " On Referring ". This article is reprinted in *Essays in Conceptual Analysis*, selected and edited by Professor Antony Flew. The references that follow are to this reprint. The main purpose of the article is to refute my theory of descriptions. As I find that some philosophers whom I respect consider that it has achieved its purpose successfully, I have come to the conclusion that a polemical reply is called for. I may say, to begin with, that I am totally unable to see any validity whatever in any of Mr. Strawson's arguments. Whether this inability is due to senility on my part or to some other cause, I must leave readers to judge.

The gist of Mr. Strawson's argument consists in identifying two problems which I have regarded as quite distinct—namely, the problem of descriptions and the problem of egocentricity. I have dealt with both these problems at considerable length, but as I have considered them to be different problems, I have not dealt with the one when I was considering the other. This enables Mr. Strawson to pretend that I have overlooked the problem of egocentricity.

He is helped in this pretence by a careful selection of material. In the article in which I first set forth the theory of descriptions, I dealt specially with two examples : " The present King of France is bald " and " Scott is the author of Waverley ". The latter example does not suit Mr. Strawson, and he therefore entirely ignores it except for one quite perfunctory reference. As regards " the present King of France ", he fastens upon the egocentric word " present " and does not seem able to grasp that, if for the word " present " I had substituted the words " in 1905 ", the whole of his argument would have collapsed.

Or perhaps not quite the whole for reasons which I had set forth before Mr. Strawson wrote. It is, however, not difficult to give other examples of the use of descriptive phrases from which egocentricity is wholly absent. I should like to see him apply his doctrine to such sentences as the following : " the square-root of minus one is half the square-root of minus four ", or " the cube of three is the integer immediately preceding the second perfect number". There are no egocentric words in either of these two sentences, but the problem of interpreting the descriptive phrases is exactly the same as if there were.

There is not a word in Mr. Strawson's article to suggest that I ever considered egocentric words, still less, that the theory which he advocates in regard to them is the very one which I had set forth at

great length and in considerable detail.[1]   The gist of what he has to
say about such words is the entirely correct statement that what they
refer to depends upon when and where they are used.   As to this, I
need only quote one paragraph from *Human Knowledge* (p. 107) :

> ' This ' denotes whatever, at the moment when the word is used,
> occupies the centre of attention.   With words which are not egocentric
> what is constant is something about the object indicated, but ' this '
> denotes a different object on each occasion of its use :   what is constant
> is not the object denoted, but its relation to the particular use of the
> word.   Whenever the word is used, the person using it is attending to
> something, and the word indicates this something.   When a word is
> not egocentric, there is no need to distinguish between different
> occasions when it is used, but we must make this distinction with
> egocentric words, since what they indicate is something having a given
> relation to the particular use of the word.

I must refer also to the case that I discuss (pp. 101 ff.) in which I am
walking with a friend on a dark night.   We lose touch with each
other and he calls, " Where are you ? " and I reply " Here I am ! "
It is of the essence of a scientific account of the world to reduce to a
minimum the egocentric element in an assertion, but success in this
attempt is a matter of degree, and is never complete where empirical
material is concerned.   This is due to the fact that the meanings of
all empirical words depend ultimately upon ostensive definitions,
that ostensive definitions depend upon experience, and that experience
is egocentric.   We can, however, by means of egocentric words,
describe something which is not egocentric ; it is this that enables us
to use a common language.

All this may be right or wrong, but, whichever it is, Mr. Strawson
should not expound it as if it were a theory that he had invented,
whereas, in fact, I had set it forth before he wrote, though perhaps he
did not grasp the purport of what I said.   I shall say no more about
egocentricity since, for the reasons I have already given, I think
Mr. Strawson completely mistaken in connecting it with the problem
of descriptions.

I am at a loss to understand Mr. Strawson's position on the subject
of names.   When he is writing about me, he says :   " There are no
logically proper names and there are no descriptions (in this sense) "
(p. 26).   But when he is writing about Quine, in MIND, October, 1956,
he takes a quite different line.   Quine has a theory that names are
unnecessary and can always be replaced by descriptions.   This
theory shocks Mr. Strawson for reasons which, to me, remain obscure.
However, I will leave the defence of Quine to Quine, who is quite
capable of looking after himself.   What is important for my purpose
is to elucidate the meaning of the words " in this sense " which Mr.
Strawson puts in brackets.   So far I can discover from the context,
what he objects to is the belief that there are words which are only

[1] *Cf. Inquiry into Meaning and Truth*, chap. vii, and *Human Knowledge*,
Part II, chap. iv.

significant because there is something that they mean, and if there were not this something, they would be empty noises, not words. For my part, I think that there must be such words if language is to have any relation to fact. The necessity for such words is made obvious by the process of ostensive definition. How do we know what is meant by such words as " red " and " blue " ? We cannot know what these words mean unless we have seen red and seen blue. If there were no red and no blue in our experience, we might, perhaps, invent some elaborate description which we could substitute for the word " red " or for the word " blue ". For example, if you were dealing with a blind man, you could hold a red-hot poker near enough for him to feel the heat, and you could tell him that red is what he would see if he could see—but of course for the word " see " you would have to substitute another elaborate description. Any description which the blind man could understand would have to be in terms of words expressing experiences which he had had. Unless fundamental words in the individual's vocabulary had this kind of direct relation to fact, language in general would have no such relation. I defy Mr. Strawson to give the usual meaning to the word " red " unless there is something which the word designates.

This brings me to a further point. " Red " is usually regarded as a predicate and as designating a universal. I prefer for purposes of philosophical analysis a language in which " red " is a subject, and, while I should not say that it is a positive error to call it a universal, I should say that calling it so invites confusion. This is connected with what Mr. Strawson calls my " logically disastrous theory of names " (p. 39). He does not deign to mention why he considers this theory " logically disastrous ". I hope that on some future occasion he will enlighten me on this point.

This brings me to a fundamental divergence between myself and many philosophers with whom Mr. Strawson appears to be in general agreement. They are persuaded that common speech is good enough not only for daily life, but also for philosophy. I, on the contrary, am persuaded that common speech is full of vagueness and inaccuracy, and that any attempt to be precise and accurate requires modification of common speech both as regards vocabulary and as regards syntax. Everybody admits that physics and chemistry and medicine each require a language which is not that of everyday life. I fail to see why philosophy, alone, should be forbidden to make a similar approach towards precision and accuracy. Let us take, in illustration, one of the commonest words of everyday speech : namely, the word " day ". The most august use of this word is in the first chapter of *Genesis* and in the Ten Commandments. The desire to keep holy the Sabbath " day " has led orthodox Jews to give a precision to the word " day " which it does not have in common speech : they have defined it as the period from one sunset to the next. Astronomers, with other reasons for seeking precision, have three sorts of day : the true solar day : the mean solar day ; and the sidereal day. These

have different uses : the true solar day is relevant if you are considering lighting-up time ; the mean solar day is relevant if you are sentenced to fourteen days without the option ; and the sidereal day is relevant if you are trying to estimate the influence of the tides in retarding the earth's rotation. All these four kinds of day—decalogical, true, mean, and sidereal—are more precise than the common use of the word " day ". If astronomers were subject to the prohibition of precision which some recent philosophers apparently favour, the whole science of astronomy would be impossible.

For technical purposes, technical languages differing from those of daily life are indispensable. I feel that those who object to linguistic novelties, if they had lived a hundred and fifty years ago, would have stuck to feet and ounces, and would have maintained that centimetres and grams savour of the guillotine.

In philosophy, it is syntax, even more than vocabulary, that needs to be corrected. The subject-predicate logic to which we are accustomed depends for its convenience upon the fact that at the usual temperature of the earth there are approximately permanent " things ". This would not be true at the temperature of the sun, and is only roughly true at the temperatures to which we are accustomed.

My theory of descriptions was never intended as an analysis of the state of mind of those who utter sentences containing descriptions. Mr. Strawson gives the name " S " to the sentence " The King of France is wise ", and he says of me " The way in which he arrived at the analysis was clearly by asking himself what would be the circumstances in which we would say that anyone who uttered the sentence S had made a true assertion ". This does not seem to me a correct account of what I was doing. Suppose (which God forbid) Mr. Strawson were so rash as to accuse his char-lady of thieving : she would reply indignantly, " I ain't never done no harm to no one ". Assuming her a pattern of virtue, I should say that she was making a true assertion, although, according to the rules of syntax which Mr. Strawson would adopt in his own speech, what she said should have meant : " there was at least one moment when I was injuring the whole human race ". Mr. Strawson would not have supposed that this was what she meant to assert, although he would not have used her words to express the same sentiment. Similarly, I was concerned to find a more accurate and analysed thought to replace the somewhat confused thoughts which most people at most times have in their heads.

Mr. Strawson objects to my saying that " the King of France is wise " is false if there is no King of France. He admits that the sentence is significant and not true, but not that it is false. This is a mere question of verbal convenience. He considers that the word " false " has an unalterable meaning which it would be sinful to regard as adjustable, though he prudently avoids telling us what this meaning is. For my part, I find it more convenient to define the word " false " so that every significant sentence is either true or false.

This is a purely verbal question ; and although I have no wish to claim the support of common usage, I do not think that he can claim it either. Suppose, for example, that in some country there was a law that no person could hold public office if he considered it false that the Ruler of the Universe is wise. I think an avowed atheist who took advantage of Mr. Strawson's doctrine to say that he did not hold this proposition false, would be regarded as a somewhat shifty character.

It is not only as to names and as to falsehood that Mr. Strawson shows his conviction that there is an unalterably right way of using words and that no change is to be tolerated however convenient it may be. He shows the same feeling as regards universal affirmatives —*i.e.* sentences of the form " All A is B ". Traditionally, such sentences are supposed to imply that there are A's, but it is much more convenient in mathematical logic to drop this implication and to consider that " All A is B " is true if there are no A's. This is wholly and solely a question of convenience. For some purposes the one convention is more convenient, and for others, the other. We shall prefer the one convention or the other according to the purpose we have in view. I agree, however, with Mr. Strawson's statement (p. 52) that ordinary language has no exact logic.

Mr. Strawson, in spite of his very real logical competance, has a curious prejudice against logic. On page 43, he has a sudden dithyrambic outburst, to the effect that life is greater than logic, which he uses to give a quite false interpretation of my doctrines.

Leaving detail aside, I think we may sum up Mr. Strawson's argument and my reply to it as follows :

There are two problems, that of descriptions and that of ego-centricity. Mr. Strawson thinks they are one and the same problem, but it is obvious from his discussion that he has not considered as many kinds of descriptive phrases as are relevant to the argument. Having confused the two problems, he asserts dogmatically that it is only the egocentric problem that needs to be solved, and he offers a solution of this problem which he seems to believe to be new, but which in fact was familiar before he wrote. He then thinks that he has offered an adequate theory of descriptions, and announces his supposed achievement with astonishing dogmatic certainty. Perhaps I am doing him an injustice, but I am unable to see in what respect this is the case.

BERTRAND RUSSELL

COMPLEMENTARY DESCRIPTIONS

In MIND, April 1956 (pp. 145-165), after a spirited attack on what he takes to be my view of the complementary relationship of scientific and religious statements, Mr. Alexander writes : " The way out of the alleged conflict seems to be through the recognition that scientific and religious statements are in different logical categories and so could not possibly conflict."

This is all rather perplexing. As long ago as 1950, when discussing the parallel case of physical and mental descriptions, I wrote as follows : " Are these rival accounts, or are they complementary ? My own view is that both are valid. The important concepts in the two accounts belong, I think, to two quite separate logical groups . . . (so that) to represent the two accounts as antithetical is fallacious."[1]

It looks as though Mr. Alexander and I are recommending very much the same view. I have no objection to his beating my drum for me ; but how comes it that I find my neck under his drumstick too ?

Alexander has of course chosen for his philosophical analysis some phrases from a loosely-worded popular talk [2]—a practice not uncommon, but fruitful of misunderstanding and wasted effort. This alone, however, does not account for the confusion. It arises, I think, chiefly from his understanding of " complementarity ", as I at least have used the term. I call two or more statements complementary when (*a*) they purport to have a common reference, (*b*) they make different allegations, yet (*c*) all are justifiable in the sense that each expresses something about the common reference which could not (for one reason or another) be expressed in the terms of the others —the commonest reason being, as I have indicated, that the terms belong to different logical categories. This is so close to what Mr. Alexander calls ' his ' definition that one naturally wonders why he objects to statements in different categories being called " complementary ".

I think the key lies in his notion of a " situation " as uniquely decomposable into, and defined by, a number of " features " (*loc. cit* p. 154). This leads him to argue that if one description can deal only with certain " features ", and another with certain others, then the two descriptions are not of the same " situation ".

This dilemma is needless if we adopt a more realistic explication of the " common reference " mentioned under (*a*) above. It may be either a collection of stable entities, or (more often, in the context of religious and scientific discussion) a dynamic spatiotemporal distribution of *events*. Either of these is what I would mean by a " situation ", and I would say that it is *aspects* rather than *com-*

[1] B.B.C. Third Programme, 16th June, 1950.
[2] B.B.C. Home Service, 2nd September, 1952. Reprinted in *Science and Faith Today*, Lutterworths, 1953.

ponents of a situation in this sense, which may find themselves mentioned in one statement, and omitted in a complementary statement, even though both statements might validly claim to comprehend the *whole* situation.

To take a well-worn example, in which the " situation " is static, the projection of a three-dimensional geometrical figure on a horizontal plane (point by point) leaves out none of the component *points* ; yet it omits all mention of the vertical *aspect* which could be revealed in an (equally exhaustive) projection of the same points on a vertical plane. (This should not be confused with the viewing of a nontransparent mountain from different angles, because in that case each view may well omit *components* represented in the other).

Here complementary statements are necessary because the logical dimensionality (2) of each " description " is lower than that of the subject (3). I would suggest this as a quite general, if somewhat ideal, criterion. Whenever the concepts available have a logical dimensionality lower than that of the subject, complementary statements are in principle required to do it justice.

The example can serve to illustrate two further points. First, in two complementary projections of a geometrical figure, a point A may well appear say to the left of B in one, and to the right of B in the other. Terms such as " to the left of " must be *labelled for standpoint* if spurious conflicts are not to arise. They are in fact defined for two *mutually exclusive logical standpoints*. This is characteristic of genuinely complementary language-systems. Each uses at least some terms whose definitions in their context presuppose logical conditions mutually exclusive of those ruling in the other(s).

Secondly, two such projections are not actually complementary (but at most supplementary) unless they refer to the same points ; and even if they do, the assertion that they are logically complementary (which is then a logical fact) says nothing as to their *validity* (which is still an empirical question). Again this is true of complementary statements in general. They are not logically complementary unless they refer to the same set of events or entities. And even when they do, their validity is still an empirical question.

How then does this concept of complementarity apply in the cases cited by Alexander ? First, in atomic physics, the basic complementarity is a mathematical one, between frequency-language on the one hand and space-or time-language on the other. A sine-wave of exact frequency extends by definition uniformly along the space- or time-axis. By definition, it has no location. Precise specification of position or epoch of a waveform is logically incompatible with precise specification of its frequency. Frequency-language and space- or time-language are logically complementary, in that an exhaustive description of a waveform may be given either in one or the other [1] but each reveals an aspect of the whole which the other necessarily omits.

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[1] Assuming the use of complex frequencies, to include phase.

This connects with Atomic Physics only through an empirical relationship between particle-momentum and wave-length, so that it cannot possibly " stifle " enquiry if properly understood. But it does mean that *as long as we accept that empirical relationship,* we cannot without self-contradiction seek to determine " particle-momentum " at a precisely specified point.

Here the experimental situations in which we must use the two languages (wave and particle) are different, illustrating again the peril of Alexander's use of " situation ". But there is of course still a common reference, namely the light or other radiation emitted by the source. The conditions for complementarity are fulfilled.

Secondly, Alexander mentions the mental and physical descriptions of a human action. The common reference here is to the pattern of *events* which together constituted the action. These, as he says, may be described from the actor-standpoint or the spectator-standpoint, and he admits (p. 164) that the corresponding descriptions may be called complementary. His objection seems to be that this suggests analogy with physical complementarity, and he points out (as I did some time ago) [1] that the wave and particle descriptions are logically too alike for the analogy to be pressed.

But of course complementarity at root is not a physical but a logical notion. It would be absurd to seek to " derive support from the physical theory " (p. 159) for the use of it in other contexts. To expect that one instance of complementarity should offer a " good analogy " for another instance is to suggest a misunderstanding of the concept. Statements either of the same or of different logical " kinds " may be complementary, as we have seen. Wave-particle complementarity can indeed be most " unhelpful " as an *analogy.* Its only merit is to illustrate the kinds of false antitheses which can arise when logical complementarity is not suspected. But the central question is not whether the comparison of mental and mechanical descriptions with wave and particle descriptions is " helpful ", but whether the complementarity of these descriptions is a logical fact. If it is, we should not allow questions of utility to obscure the fact.

Finally, what of Mr. Alexander's criticisms of the concept as used in relation to scientific and religious statements ? Again he raises (and sometimes mixes) objections on different grounds : (*a*) the concept must introduce inconsistencies (p. 151) (*b*) the concept is not useful (for a variety of purposes). I hope it is now clear that if the " common data " are taken to be *events*, rather than " features " (whatever these may be) there is in fact no inconsistency in saying that descriptions in religious and scientific categories arise from different approaches to the same data, each in principle being exhaustive in the sense that no events need be missed in either, yet

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[1] " Man as Observer-Predictor ", in *Proc. of Present Question Conference,* 1954, p. 23 (Routledge and Kegan Paul, 1955.)

each saying something that the other logically cannot in its categories, and so *complementing* the account given by the other. Alexander's analysis shows usefully how inconsistency could arise if different definitions of terms were given. But these definitions are not mine, and those which I use do not I think " do violence to language " in any of the respects he mentions.

Granted that it entails no inconsistency, I think the recognition of this complementarity is useful for two reasons. First, it should prevent both supporters and antagonists from seeking to relate scientific and religious statements in wrong (*i.e.* logically inadmissible) ways. This could of course be done by a flat assertion that their categories are different ; but by itself this might seem to encourage or condone a " watertight—compartment " mentality which is potentially dishonest and theologically non-Christian.

Hence, to my mind, the second reason for insisting on the complementarity of the two types of statement. According to Christian doctrine they do have a common reference, in the sense that every scientific statement has as its factual basis a set of events which in principle admit of complementary accounts in religious categories (though not necessarily conversely). God is declared to be immanent in all events, as well as transcendent. One may of course question this doctrine ; but that is another matter. I am defending only the claims of " complementarity " to be a valid and useful representation of the relationship which it implies between religious and scientific statements. While guarding against efforts at a " synthesis " of the wrong kind, it yet offers a positive encouragement to the scientist who is a Christian to seek to integrate his thinking properly in the two domains.

One " use " which Mr. Alexander seems to expect for complementarity it certainly does not have. It does—and should do— nothing in itself to demonstrate the *truth* of the Christian position to a non-Christian.

Argument—including argument about complementarity—has its place in demonstrating that Christianity entails no inconsistencies : in clearing away preliminary obstacles. But the core of Christian conviction is not an argument but a growing complex of experience of a new kind—the outcome of what is called repentance, surrender, and all that goes with them. Like all experience it is of course personal and private ; but it does not thereby escape discipline and cross-checking ; both the experience of fellow-Christians and the body of Christian theology provide those in salutary measure.

By calling it a new kind of experience Christians do not of course imply that they have special sensory or psychological equipment. What is new is the pattern that experience takes on if one is prepared for it to do so. The ultimate barrier—and the ultimate gateway—to conviction of this sort, as in other matters of perception, is attitudinal, as Christ himself pointed out (*Mark* 10 : 15). It would be a theological blunder, and no merit as Alexander seems to think, if

" the Theory " did not " leave open the question of the necessity for the religious description " (p. 160).

But when all misunderstanding has been set aside, there is much in what Alexander has said with which I agree. Perhaps I may best indicate this agreement by quoting in conclusion a few excerpts from a discussion in 1953 on the same topic.[1]

" Whenever a new concept swims into philosophical ken there is a danger that it will be overworked by the Athenians on the one hand and abused by the Laodiceans on the other. Complementarity is no universal panacea, and it is a relationship that can be predicated of two descriptions only with careful safeguards against admitting nonsense. Indeed the difficult task is not to establish the possibility that two statements are logically complementary, but to find a rigorous way of detecting when they are not. . . . A good deal of consecrated hard work is needed on the part of Christians to develop a more coherent and more biblical picture of the relationship between the two . . . But if once we recognize that at least most theological categories are not ' in the same plane ' (in the same logical subspace) as most scientific categories, there is no longer any theological merit in hunting for gaps in the scientific pattern. Gaps there are in plenty. But it would seem to be the Christian's duty to allow—indeed to help—these gaps to fill or widen as they will, in humble and cheerful obedience to the truth as God reveals it through our scientific discipline, believing that to have theological stakes in scientific answers to scientific questions is to err in company with those unbelievers who do the like."

Mr. Alexander may find some anticipation here of his own concluding remarks. But if we refuse, with him, to " prefer comfort to industry ", it still behoves us to take care that we " break our heads " on the right problems.

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[1] D. M. MacKay, *The Christian Graduate*, December 1953, pp. 163-164.

ON AN APPARENT CIRCULARITY IN SOME
DEFINITIONS OF LOGICAL TRUTH [1]

In his excellent review paper of Strawson W. V. Quine suggests that instead of defining " the province of logic " by use of the notion of analyticity as Strawson does, the following might have done : " he could have used, instead, the notion of truth and the notion of logical vocabulary. Given these, the business of formal logic is describable as that of finding statement forms which are *logical*, in the sense of containing no constants beyond the logical vocabulary, and (extensionally) *valid*, in the sense that all statements exemplifying the form in question are true. Statements exemplifying such forms may be called *logically true* ".[2]

This definition is identical with what Quine has expounded elsewhere in somewhat more technical terms. As it easily furnishes us with a way of defining the concept of logical consequence by saying that a certain statement is a logical consequence of another if the former exemplifies a statement form S and the latter another statement form S' such that whenever all statement exemplifications of S' are true all statement exemplifications of S are true too, Quine's definition seems to differ in no essential way from Tarski's [3], but rather subsumes it as a special case.

The purpose of the present note is to inquire what notion of truth, together with the notion of logical vocabulary, is occurring in this suggested definition of logical truth. Evidently it cannot be logical truth, but must be some other kind of truth. The only one of which I know is the notion of truth we use when we say that it is true that Boston is north of New York, grass is green, and Shakespeare the author of " Hamlet ", material truth to wit.

Consider now the statement " it rains or it does not rain ". It is logically true, indeed, exemplifying the tautologous form " $p \vee \sim p$ " and furthermore it has to be true in the second sense too, the sense used in the definition of logical truth.

Now it seems to me that it is fair to state without elaborate discussion that to claim truth is senseless if no procedure of verification is possible. Let us not, however, be too strict about verification, we are willing to include paper and pencil operations in Bridgman's terms.

So we must ask what methods of verification are available for establishing material truth of " it rains or it does not rain ". These methods ought to be different from the methods we use in showing the statement in question logically true. That they ought to be so is

[1] Discussions with Professor Jørgen Jørgensen stimulated this note, although he, perhaps, will not agree with my line of thought.

[2] MIND, October 1953, p. 436.

[3] See his " On the Concept of Logical Consequence ", paper XVI in " Logic, Semantics, Metamathematics ", Oxford, 1956.

immediately clear, I hope, wholly independently of any more or less questionable theory about the connexion between meaning and verification.

To verify conclusively the logical truth of " it rains or it does not rain " it is sufficient to point out that we have to do with a statement exemplifying a tautology " p v ∼ p ". But how to verify the material truth of the same statement ? Our first reaction to this question naturally will be an attempt to apply the usual methods of verification for material truths. In the particular case we will discover, by looking out of the window, that it is materially true that it rains, or we will discover that it is materially true that it does not rain. To verify in such a way, by investigating the world, is exactly similar to the procedure of verification of which the statements " Boston is north of New York " etc. are susceptible.

Unfortunately it will furnish us with the truth of only one of the components of our disjunctive statement. So it must be supplemented, and by further applying truth-value analysis, a particular kind of paper and pencil operation, we easily discover that the statement is materially true, every disjunction being materially true if only one component is materially true.

In submitting a statement to a truth-value analysis, however, we obviously presuppose that every statement is either true or false. If more possibilities than these were open we simply could not use the truth-tables, at least not in their ordinary form. But this is exactly what says the so-called law of the excluded middle of which " it rains or it does not rain " is an exemplification.

To quote Quine : " ⌜ φ v ∼ φ ⌝ illustrates the law of the excluded middle, which is commonly phrased as saying that every statement is true or false." This law is " not to be identified with ⌜ φ v ∼ φ ⌝ " itself, for the latter is an unspecified statement " pending specification of φ ", and " become the minor truth ' Jones is ill v ∼ Jones is ill ' . . . when φ is specified as ' Jones is ill '. But the law of the excluded middle may be formulated as saying that ⌜ φ v ∼ φ ⌝ is true for every statement φ . . .".[1]

Accordingly the law of the excluded middle tells us the very same as the assertion that the statement form " p v ∼ p " is tautologous. In establishing the logical truth of " it rains or it does not rain " we appeal directly to the latter assertion, in establishing the material truth of this statement we indirectly make use of the same principle in the truth-value analysis.

Although the procedures of verification are thus not strictly identical, material truth of our statement cannot be established without appeal to the very same tautologous form " p v ∼ p " that was used in showing it to be a logical truth.

Consequently " material truth " seems to me to be an empty term

[1] Mathematical Logic, Cambridge, 1951, p. 51. The terminology of quasiquotation is not essentially, but only notationally different from that of statement forms.

when applied to " it rains or it does not rain ", as there is no way of verifying this truth without invoking the tautological statement form, which it exemplifies, and which immediately tells us that it is logically true.   Accordingly we cannot properly speak of the truth of the statement in question without meaning its logical, formal truth, or perhaps analytic truth.   We realize its truth only by taking into consideration its form, and if the preceding analysis is right any talk of its material truth is simply senseless, until defined in such a way that different and independent verification is possible.   Otherwise the definition of logical truth in terms of material truth plus logical vocabulary is open to a charge of circularity.

I have illustrated the circularity by a particular example.   If we will be involved in a similar circularity with respect to other logical truths than those of the form " p v $\sim$ p " I do not know for certain, but I would consider it probable.   That the proposed definition of logical truth is defective has, however, no influence on the impressive body of logical truths assembled in the works of Quine and other mathematical logicians, nor, of course, on his other points against Strawson.

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A DISCOVERY IN TRADITIONAL LOGIC

EVERY logician knows the so-called Aristotelian Sorites. One simply takes the predicate of the first statement and makes it the subject of the second statement, and makes the predicate of the second the subject of the third, and so on, the first subject being the subject of the conclusion and the last predicate being the predicate of the conclusion. The rules of this Sorites are that (i) the first premise may be particular, but the rest must be universal; (ii) the last may be negative, but the rest must be affirmative. Schematically it can be represented as follows :

$$aA/Ib$$
$$bAc$$
$$cAd$$
$$dA/Ee$$
$$\overline{}$$
$$aAEIOe$$

As an actual example one might cite a development of Berkeleyan Idealism and as a second a development of Realism.

> All spirit is substance ;
> All substance is a possible object of thought ;
> All possible objects of thought are sense-given :
> All sense data are created by the sentient mind.
> ∴ All spirit is created by the sentient mind.

All spirit is substance ;
All substance is a possible object of thought ;
All possible objects of thought are sense-given ;
No sense data are created by the sentient mind.
∴ No spirit is created by the sentient mind.

Now this is simply an enlargement of the Syllogism of the First Figure, with the minor premise placed first. Thus the rules for this First Figure are (i) the minor must be affirmative ; (ii) the major must be universal. That is, the minor, as affirmative, may be either A or I ; the major, as universal may be either A or E. So in the Sorites the first premise (= minor) must be affirmative according to rule (ii), but as rule (i) allows it to be particular, it may be either A or I ; the last premise (= major) must be universal according to rule (i), but rule (ii) allows it to be negative as well as affirmative, and so it may be either A or E. This agrees exactly with the rules for the First Figure, and hence this Sorites is, in fact, nothing more than an enlargement of the First Figure Syllogism.

The question arises : Are there also Sorites corresponding to the Syllogisms of the Second and Third Figures ? I omit the Fourth Figure, because this is really only an elaboration of the First. The

Goclenian Sorites is really only the Aristotelian with the terms differently arranged.

If we start off with the major and minor of a Syllogism of the Third Figure, placing the minor first, then the first predicate becomes the third subject, the second predicate becomes the fourth subject, and so on. The second last predicate gives the subject of the conclusion and the last predicate gives the predicate of the conclusion. All intermediate terms must be universal affirmative, but the first two terms must be either A and I or I and A respectively; the last term must be universal, either affirmative or negative. The conclusion must be particular, either affirmative or negative depending on the value of the last term. This will be clearer from the schematic example below. But first we shall couch the rules for this Sorites in more conventional terms, thus : (i) either the first or the second term (but not both) may be particular, but all the rest must be universal ; (ii) The last term may be negative, but all the rest must be affirmative ; (iii) The conclusion must be particular. Here is the scheme :

$$aA/Ib$$
$$aI/Ac$$
$$bAd$$
$$cA/Ee$$
$$\overline{dI/Oe}$$

As an actual example we might cite a development of Realism, the same position, in fact, as in the Aristotelian Sorites, for to confess the truth this Sorites does not give any conclusions different from the Aristotelian, but only uses premises which are suppressed in the other :

All spirit is substance ;
All spirit is a possible object of thought ;
All substance is sense-given ;
No possible objects of thought are created by the sentient mind.
∴ Some sense data are not created by the sentient mind.

It can be easily demonstrated that this merely elaborates the Third Figure Syllogism. For all the terms, except the first, are majors, and rule (i) asserts that these must be universal and rule (ii) that they must also be affirmative. This is so that the intermediate terms resulting from each step, which are minors, will be affirmative. So the rules of the Third Figure syllogism are (i) the minor must be affirmative, and (ii) the conclusion must be particular, which latter is covered by rule (iii) of the Sorites.

Next, the Sorites of the Second Figure bears a general resemblance to this, but is in reverse order. That is, we start off with a Second Figure major and minor in that order ; then the first subject gives the third predicate, the second subject gives the fourth predicate,

and so on, with the conclusion having its subject and predicate from the last and second-last subjects respectively. All intermediate terms must be universal affirmative, the first two, however, being either A and E or E and A respectively. The last may be a particular affirmative, and the conclusion must be negative, either particular or universal depending on the distribution of the last term. This may be expressed more succinctly as follows : (i) either the first or the second term must be negative, but all the rest must be affirmative ; (ii) the last term may be particular, but all the rest must be universal. Here is the scheme :

$$dE/Ae$$
$$cA/Ee$$
$$bAd$$
$$aA/Ic$$

$$\overline{aE/Ob}$$

As an example I cite a development of Parmenides :

    All sense data are created by the sentient mind ;
    No possible objects of thought are created by the sentient mind ;
    All substance is sense-given ;
    All spirit is a possible object of thought.
    ∴ No spirit is substance.

Now rule (i) ensures that one term is negative right through, and rule (ii) that all except the last will be universal. As all terms except the first are minors, rule (ii) means in effect that the majors will be universal. So the rules of the Second Figure Syllogism are (i) one premise must be negative, (ii) the major must be universal. Unlike the preceding Sorites, this gives a position different from the Aristotelian Sorites, and so is a discovery in traditional logic.

<div align="right">H. B. PHILLIPS</div>

IX.—CRITICAL NOTICE

Logic, Semantics and Metamathematics : Papers from 1923 to 1938.
By Alfred Tarski. Translated by J. H. Woodger. Oxford,
Clarendon Press, 1956. Pp. ix + 471. £3.

I suppose that most philosophers who pick up this book will make a
bee-line for Paper VIII, the famous ' Truth ' thing ; but this is a
pity. For Paper I, which is substantially Tarski's doctoral disserta-
tion of 1923, is not only a singularly brilliant piece of formalism but
bears upon some serious philosophical problems.

The question with which this first paper opens is a technical one
arising in the discipline which Tarski's teacher Leśniewski came to
call ' protothetic ', *i.e.* propositional calculus enriched with variables
standing for statement-forming operators on statements (like ' It is
not the case that—— ') and with quantifiers binding both these and
variables of the ordinary type standing for statements. If defini-
tions are introduced into such a calculus as axiomatic universal
equivalences (a practice on which Leśniewski insisted), there is an
obvious advantage in making the symbol for ' if and only if ' the one
primitive constant (apart from the universal quantifier) of the
system—the same symbol then becomes at once the instrument and
the ultimate matter of all definitions. And the problem of defining
all other truth-functions in terms of equivalence can be fairly
straightforwardly solved if such a definition can be found for the
simple ' and '. Tarski gives such a definition (of ' and ' in terms of
' if and only if ' and the universal quantifier) ; in fact he gives two,
and the difference between them constitutes, to my mind, the main
present interest of this paper.

The first definition equates the form ' p and q ' with the following :

A. For all f, p if and only if (for all r, p if and only if $f(r)$) if and
only if (for all r, q if and only if $f(r)$).

This is an involved formula, but its formal equivalence to the simple
' p and q ' (and consequent suitability for use as a definition of this)
is proved quite straightforwardly, in three stages. It is shown first
that A implies q, then that A implies p, so that A implies p-and-q,
and then conversely that p-and-q implies A. The three proofs are
as follows:

Proof that A *implies* q.—Let $tr(r)$ be the tautological function ' r
if and only if r '. Then A implies, by instantiation of f,

B. p if and only if (for all r, p if and only if $tr(r)$) if and only if
(for all r, q if and only if $tr(r)$).

But the contained form ' For all r, p if and only if $tr(r)$ ' equates
p's truth-value with that of a tautology, and so is tantamount to

the simple assertion of p as true. ' For all r, q if and only if $tr(r)$ '
is similarly tantamount to the assertion of q as true.   Hence B can
be simplified to

C. p if and only if (p if and only if q) '.
By the commutability and associativity of equivalence, this is
equivalent to

D. q if and only if (p if and only if p),
which equates q's truth-value with that of a tautology, and so implies
q.   Hence A implies q.

Proof that A *implies* p.—The plain r is a function of itself (or, if
you like to be fussy, is equivalent to the function ' It is the case that
r '). Hence A implies, by instantiation of f,

E. p if and only if (for all r, p if and only if r) if and only if (for
all r, q if and only if r).
But there are statements, *e.g.* Not p, to which p is not equivalent.
Hence ' For all r, p if and only if r ' is simply false ; similarly ' For
all r, q if and only if r ' is simply false, so that the two have the same
truth-value, and E equates p with a truth.   Hence E, and therefore
A, implies p.

Proof that p-*and*-q *implies* A.—The joint truth of p and q implies
their equivalence (their having the same truth-value), and therefore
implies that if either is equivalent to anything, say $f(r)$, the other is
equivalent to it too.   That is, p-and-q implies, for any f and r.

F. (p if and only if $f(r)$) if and only if (q if and only if $f(r)$).
And this implies, for any f,

G. (For all r, p if and only if $f(r)$) if and only if (for all r, q if and
only if $f(r)$).
But ' p and q ' also implies the plain p, and by thus implying that
G and p are both true it implies that they have the same truth-
value ; that is, it implies A.

I would recommend for most readers a similar verbalising of the
other proofs in this paper ; the results are worth the trouble.   Two
points that should be clear about the last proof in particular are
these : (i) It depends heavily on the ' material ' or truth-functional
interpretation being given to the specific form ' p if and only if q ',
or ' p is equivalent to q '.   But (ii) it does not depend in the least
in the general form $f(r)$ being restricted to truth-functions of r.   It
does, of course, cover these (so that the truth-functions $tr(r)$ and r
can be treated as instances of this form), but not these only.   Our
formula F, for example, will follow from the equivalence of p and q
even if $f(r)$ is taken to be such a function as ' Ptolemy believed
that r ', ' It is possible that r ', ' It is a bad thing that r ', and so on.
And it is just this which distinguishes this definition of ' and ' from
Tarski's second one, which equates the form ' p and q ' with

H. For all f, p if and only if ($f(p)$ if and only if $f(q)$).

This is a vastly simpler *definiens* than A ; but suppose we let p be ' Athens is in Greece ', q ' The earth goes round the sun ' and f the expression ' Ptolemy believed that—— '.   Since Ptolemy believed that Athens is in Greece but not that the earth goes round the sun, ' $f(p)$ if and only if $f(q)$ ' is in this instance false ; but p is true, so that H as a whole is falsified.   The allegedly equivalent form ' p and q ' is, however, true.   It is therefore not surprising that Tarski's proof of the equivalence of ' p and q ' to H should require a premiss which in effect restricts $f(p)$ and $f(q)$ to truth-functions ; namely the premiss

J. For all p, q and f, if both (p if and only if q) and $f(p)$, then $f(q)$,

i.e. if p and q have the same truth-value then q may replace p, without loss of truth, in any context.

Tarski makes it clear in passing that his own view is that J is not merely a postulate which restricts our universe of discourse on a particular occasion to truth-functions, but is a simple and universal truth—there really are, in his view, no functions of statements except truth-functions.   What he makes of, *e.g.* ' Ptolemy believed that Athens is in Greece ' he does not say ; but one may surmise that he regards this as more accurately expressed by ' Ptolemy believed " Athens is in Greece " ', or by something else which is not a function of a statement but rather of a name of a statement.   My own view is that all such reductions of ' non-extensional ' functions are utterly unplausible, and that ' Ptolemy believed that Athens is in Greece ' is not in any sense a statement about the statement ' Athens is in Greece ', but a new statement, constructed of this latter, which is also about Athens and Greece.   But I do not want to pick a quarrel with Tarski about that now ; I want, rather, to commend the superb detachment with which, despite this belief, Tarski insists that J is not itself deducible from ordinary truth-functional logic, and proceeds to sort out those theorems involving variables like f which depend on J (or on some equivalent postulate—he gives us several) from those which do not.   He has in this way put all of us, ' extensionalists ' and ' non-extensionalists ' alike, in his debt ; and his paper makes most other writings on both sides seem disreputable by comparison.   One point, though, that he fails to notice is that in systems which do not contain the postulate J, axiomatic equivalences do not function quite as definitions do, since without J we cannot pass from such an equivalence to the interchangeability of *definiens* and *definiendum* in all contexts.

The same skill in exhibiting what depends on what is shown in Papers II, ' Foundations of the Geometry of Solids ' (1927), and XI, ' On the Foundations of Boolean Algebra ' (1935), in which different superstructures are built upon another Leśniewskian discipline, the part-whole calculus or ' mereology '.   The relation of a class to its sub-classes is a special case of that of a whole to its

parts, and Tarski distinguishes very exactly in XI between those postulates of the calculus of classes which simply reflect this fact and those which reflect the special features of this particular sort of whole ; and within the latter, between those which merely reflect the existence of a ' null class ' which is a part—in the relevant sense —of every class, and those which reflect the way in which the subdivision of non-empty classes always finishes up with ' unit classes ' which have no parts but the null class and themselves.

The calculus of classes, again, is one of the presuppositions of the discipline with which Tarski's name is more closely associated than any other, ' formalized metalogic ' or the study of classes of sentences ordered by the relation of deducibility. This topic is introduced in Paper III, ' On Some Fundamental Concepts of Metamathematics ' (1928), and continued in V, ' Fundamental Concepts of the Methodology of the Deductive Sciences ' (1930), and XII, ' Foundations of the Calculus of Systems ' (1935-36). A deductive system is defined as a set of sentences which coincides with the set of its consequences (every sentence is a consequence of itself, and a deductive system contains all the consequences of all sentences in it). Not all deductive systems are ' axiomatizable ', but only those which contain some finite set of sentences from which all the rest are deducible. Such conceptions as these, and related conceptions such as those of consistency, completeness and independence, are given symbolic representation, and statements involving them are themselves organized into axiomatized deductive systems. This material is rough going for the non-mathematician, but it is worth at least a look from the philosopher, since it underlies so much else that Tarski writes, not including I, II or XI, but including IV, ' Investigations into the Sentential Calculus ' (with Łukasiewicz, 1930), XIV, ' On Extensions of Incomplete Systems of the Sentential Calculus ' (1935), and parts of VIII, ' The Concept of Truth in Formalized Languages ' (1931). One tip for the reader unused to the symbolism : The form ' $Y = X \mathrel{C} S$ ' means ' The set Y coincides with the set X, which is included in the set S ', and similarly in other cases ; and the form ' There is a set $Y \mathrel{C} S$ ' means ' There is a set Y which is included in the set S ' (and ' There is an $x \in Y$ ', ' There is a sentence x which is a member of the set Y ').

Some of the variations in the postulates used for the calculus of systems are interesting. In III and V the function $Cn(x)$, the set of consequences of the set of sentences X, is taken as primitive, and L, the set of logically true sentences, is defined in terms of this as $Cn(O)$. (O being the set included in every set, this amounts to defining logically true sentences as those which follow from any premisses whatever). In XII the set L is taken as primitive and the relation Cn defined. But the later procedure is only available when the totality of sentences S with which we are concerned contains implications, since Cn is defined by saying that y is a consequence of the set X if either it is itself in L or its implication by the logical product of

some sub-set of X is in L. The resulting total postulate-set is simplified by this change, but it precludes something which Tarski effects very beautifully at the beginning of III, namely the clear separation of those postulates of the calculus of systems which simply reflect the properties of the consequence-relation as it might occur in any language whatsoever, from those which reflect the special properties of particular languages (in this paper, of languages which contain implications and negations).

The precise postulates that he uses for these two purposes are worth spelling out and playing with a bit. He has five of each, but the fifth of the first sort, that there is a sentence x in S such that its consequences coincide with the whole of S, seems to me misplaced—it belongs rather to ' special ' metalogic, and is in fact dropped by Tarski in V, where ' general ' metalogic alone is discussed. Axioms 2 and 3, typical of the first group, state that any set X is included in the set of its consequences, and that the consequences of the consequences of X are included in its consequences. Of the second group, Axiom 6 states that if x and y are sentences in S, so is the implication of y by x ; 7 and 8 that if and only if the implication of y by x follows from the set of sentences X, the plain y follows from the set consisting of X with x added ; 9 that the consequences of the set consisting of x together with its negation comprise the whole of S. Among the theorems provable from Tarski's postulates is that all sentences exemplifying laws of the propositional calculus are in the set $Cn(O)$. The actual proof of this is left to the reader ; here, for a start, is the proof of Łukasiewicz's axiom $CpCNpq$: Write $n(x)$ for ' the negation of x ' and $c(x, y)$ for ' the implication of y by x '. By Tarski's 9, y follows from the set consisting of x and $n(x)$; hence, by 8, $c(n(x), y)$ follows from the set consisting of x only ; hence, by 2 and 3, $c(n(x), y)$ follows from any set containing x, and so from that consisting of x together with any sentence z ; hence, by 8, $c(x, c(n(x), y))$ follows from z alone, z being anything at all ; that is, any sentence of the form $CpCNpq$ is in $Cn(O)$.

All this is worth comparing with the papers on deducibility by Popper which came out in the late forties. There is a similar initial statement of what is true of deducibility quite generally ; but the special axioms for deductions in which implications and negations are concerned are replaced by ' inferential definitions ' of implication and negation signs. For example, Popper says in effect that a sign c occurring in sentences of a language S is an implication sign if the sentence $c(x, y)$ follows from a set of sentences X if and only if the plain y follows from the set consisting of X with x added. The results are much as before, and Popper's way of putting the thing seems a pointless complication. It is said to rid logic of ' assumptions ' ; but suppose we take one of these inferential definitions seriously and use it to determine whether a given word, say ' plok ', is being used in a language S as an implication sign. To use the definition at all, we must know independently what is meant

by the sentence which consists of two sentences preceded by ' plok ',
and we must have enough logic to know whether this sentence
follows from given premisses when and only when the second con-
tained sentence follows from those premisses with the first contained
sentence added to them. We must, in short, have precisely the
knowledge, or make precisely the assumptions, which Tarski ex-
presses in his seventh and eighth axioms.

   One objection which may be made against Tarski, and perhaps
against Popper too, is that far too much is said in this context about
' sentences '. It is not so much sentences which are premisses and
conclusions, as what the sentences mean ; or at all events whether
a given sentence follows from another or others depends on what
they mean, and not on how they are put together (except so far as
this conditions what they mean). How far Tarski would go to meet
this objection I do not know, but that he would go at least a little
way is indicated in Paper XVI, a comparatively informal piece ' On
the Concept of Logical Consequence '. Here he makes deducibility
an essentially ' semantical ' concept, built up from the notion of a
set of real objects which ' satisfy ' certain characterisations (' senten-
tial functions '). Roughly, y is a logical consequence of x if any set
of objects which satisfies any characterisation with the logical
structure exemplified by x also satisfies the corresponding character-
isation with the logical structure of y. This presupposes a division
of the expressions occurring in sentences into those which determine
their logical structure and those which give them their non-logical
content. This division, Tarski says, is ' not quite arbitrary ', but
he knows no way of making it quite absolute either, and thinks
that we must be prepared to find that whether y is to be counted
as a logical consequence of x depends on a previous and partly
arbitrary decision as to what expressions we shall count as logical.
Some writers, he notes, make use of ' the concept of *tautology* (*i.e.*
of a statement which " says nothing about reality ") ' to mark off
logically true propositions (and so to mark off logical consequences
by the method of Tarski's XII) ; but ' to me personally ' he says,
this concept ' seems rather vague '. (I can only register the same
reaction.) What we can determine quite definitely is whether a
given conclusion follows from given premisses by an explicitly
specified rule or rules, and it is in settling questions of this sort that
Tarski's skill is shown again and again in these papers ; but Gödel's
incompleteness theorem compels him to dismiss the possibility of
defining the general notion of logical consequence in terms of such
rules.

   And now for Truth. This paper (VIII) begins by discussing
' The Concept of True Sentence in Everyday or Colloquial Language';
and since such phrases as ' everyday language ' tend at the moment
to be battle-cries, readers will be in some danger of chasing red-
herrings. (Tarski too, perhaps.) We generally say that a sentence is
true if it says that something is so, and it is so ; but that this needs

to be made more precise is indicated by the paradox of the liar. Tarski tries the formulation ' For all p, " p " is a true sentence if and only if p ', and this does indeed avoid the paradox of the liar, but only by being itself flagrantly false in another way—'p' in quotes is the name in the 16th letter of the alphabet, which is not a true sentence under any conditions at all. And now comes a stiff bit, but an important bit—more important, I think, than Tarski himself realises. He says that there might be a way of using quotes in which you can substitute actual sentences for sentential variables occurring within them—in which, in other words, a variable in quotes is not the name of a variable but a variable name. But if you do this, he says, and interpret the quotes in the definition of truth in this way, the paradox of the liar returns.

I am not at all sure that this is really so. In everyday language we often dispense with quotes in favour of ' that ' clauses, and Tarski's second way of using quotes is something very like the ordinary use of ' that ' clauses. Now it would be generally agreed that for all x and p, if x is a sentence asserting that p, then x is true if and only if p. (This is not exactly a definition of ' true ', but it might be expected to follow from one.) Let the sentence ' The sentence in the square is not true ' be written in a square (and nothing else be written there). The sentence in the square, we may then say, is a sentence asserting that the sentence in the square is not true ; and from this it follows, by the principle just stated, that the sentence in the square is true if and only if the sentence in the square is not true. So we do seem to obtain the paradox of the liar by using ' that ' clauses instead of quotation-marks. But can we not solve it by saying that on reflection it is clear that the sentence in the square does not assert anything at all ? We might support this by arguing that a sentence only makes an assertion if it refers to some matter of fact (in quite a broad sense—logical or grammatical facts, for example, would count) which determines whether it is true or false ; but whether the sentence in the square is true or false could only be determined (that sentence being what it is) by whether the sentence in the square is true or false, which violates the one-way character of the determination involved, so that nothing is asserted.

Tarski's reply to this would be to say that the only way of showing that a sentence makes no assertion is by showing that either (a) its component expressions have had no meaning assigned to them, or (b) its component expressions have not been put together according to the rules of the language to which it belongs. And neither of these things can be said about ' The sentence in the square is not true '. From which it is concluded that in the language to which the sentence belongs, namely ' everyday or colloquial language ', the paradox cannot be avoided, so that that language is inconsistent. I do not personally find this conclusion at all unpalatable—I do not see why everyday or colloquial language, which is not after all the

creation of a logicians' congress, should not turn out to be inconsistent at this point or that—but Tarski's proof of it is not, to me, convincing. For everyday or colloquial language just isn't the sort of language in which the sense of its sentences is entirely determined by the sense of their parts and the ways in which these parts are put together. It is a definition of truth which can be applied to this last sort of language that Tarski is seeking ; when that is understood, and only when that is understood, his achievement can be seen in its right proportions. And, of course, a very considerable achievement it is ; for in languages of this sort the avoidance of the ' Liar ' paradox presents all the difficulties which Tarski says it does.

His first result is that a definition of truth which is to be applicable to a ' formalized ' language cannot in general be constructed within that language itself ; it requires a metalanguage.   He then proceeds to give an actual definition for a comparatively simple language containing symbols for classes, for the relation of inclusion between classes, for ' or ' and ' not ' and for the universal quantifier. Skipping some important refinements, the essence of the dodge is this : A ' sentence ' is defined as a special sort of ' sentential function ' (one without free variables), and ' truth ' as the corresponding special case of the ' satisfaction ' of a sentential function.   ' Satisfaction ' is so defined that the function ' ϕx ' will be said to be satisfied by the class a if and only if ϕa, and to be satisfied by all classes whatever if and only if $\Pi x \phi x$. Consequently ' $\Pi x \phi x$ ' is satisfied by all classes whatever if and only if $\Pi x \Pi x \phi x$; but the initial quantifier being vacuous in this case, this amounts to saying that ' $\Pi x \phi x$ ' is satisfied by all classes whatever if and only if $\Pi x \phi x$, and such universal satisfaction of a function without free variables is equated, in the final definition, with truth. This definition of Tr (the class of true sentences of the language studied) is no doubt highly artificial, but it does meet the primary condition of having as its consequences (given an ordinary metalogical postulate-set)

> ' all sentences which are obtained from the expression " $x \epsilon Tr$ if and only if p " by substituting for the symbol " x " a structural-descriptive name of any sentence of the language in question and for the symbol " p " the . . . translation of this sentence into the metalanguage '

(deducibility of these is laid down by Tarski as his ' Convention T ' for judging any proposed definition of Tr). It also gives rise to such desirable theorems as that a proposition and its negation are never both true, that whatever is provable is true, and so forth.

When the presupposed definition of ' satisfaction ' is examined, however, it will be found that this definition of truth has a further defect. Satisfaction can only be defined in the following roundabout way (I again give the thing roughly) : ' x is included in y ' is satisfied by the pair of classes a, b if and only if a is included in b ; ' not-y ' is satisfied by any group of classes which does not satisfy

y ; ' *x* or *y* ' is satisfied by any group of classes which either satisfies
x or satisfies *y* ; and a function preceded by the universal quantifier
is satisfied, etc. Such a piecemeal definition of satisfaction means a
similarly piecemeal definition of truth, when it is all spelt out ; and
the more complex the language considered the more pieces there
will be. I know there are plenty of quite un-Tarski-like people who
will be entirely happy about this—people who contend that even in
' everyday or colloquial ' language the word ' true ' has different
meanings when applied to sentences of different sorts, so that it
can have a single meaning only in the sense of a disjunction of these.
My own understanding of ordinary language is quite otherwise ;
there are no doubt dozens of different ways of deciding whether a
given sentence is true, but what it means for a sentence to be true
is pretty much the same throughout, and pretty much what was
suggested at the beginning of this discussion. So when in the for-
malized metalogic of a formalized language a disjunctive definition
is given, I can only swallow this as part of the artificiality which
definitions in symbolic calculi are apt to have anyhow (like defining
' not *p* ' as ' neither *p* nor *p* ' or as ' If *p* then anything you please ',
or ' and ' as that thing in Paper I). For the matter of that, it is
not really necessary to define *Tr* in a formal system at all ; one
could take it as primitive and just lay down axioms for it, and
Tarski shows that with formalized languages of a certain peculiar
complexity we cannot do anything else. We could in fact sum up
his investigations into this whole matter by saying that even when
it is done in a metalanguage (as it must be), no definition of truth for
a formalized language will be much like the natural one ; the nearest
we can get to that is not a definition but a meta-metalogical rule,
permitting us to lay down as metalogical axioms all sentences of the
sort described in the Convention T.

   Most of the remaining papers fall far more definitely into the pigeon-
hole of ' metamathematics ' than into either ' logic ' or ' semantics ',
but a notable exception is IV, ' Investigations into the Sentential
Calculus '. This, written jointly with Łukasiewicz, summarises
results gained in the 1920's by Lindenbaum, Sobociński, Wajsberg
and the authors, concerning the ordinary two-valued sentential cal-
culus with *C* and *N* as primitives ; Łukasiewicz's many-valued
systems in *C* and *N* ; the ' restricted ' sentential calculus with no
constant but *C* ; and the ' extended ' sentential calculus in which
C is supplemented by the universal quantifier binding sentential
variables (sentential calculus with ' functor '-variables, as in I, is
not considered). No proofs are given, and the statement of results
is very compact, but with a little effort one can tease out of the
symbols a magnificent record of individual and collective achieve-
ment ; and it leaves one with an itch to see the collected papers of
Łukasiewicz, Wajsberg and Sobocinski. Some of the results have
by now become commonplaces, but many have not, *e.g.* the method
given for constructing a truth-value matrix for any *n*-valued

propositional calculus in C and Π (where n is finite). The truth-conditions for C are as in the n-valued C-N calculus, and the value of $\Pi p f(p)$ is found by equating this to the logical product of $f(g_1)$, $f(g_2) \ldots f(gn)$, where $g_1, g_2 \ldots gn$ are a series of propositional constants (constructed by means of C and Π) taking each of the n truth-values. Some simple abbreviations would have helped most readers at this point. In all the C-Π systems, $O = \Pi pp$ and $Np = CpO$; in the 2-valued system, $1 = \Pi pCpp$; in the 3-valued, $\frac{1}{2} = \Pi pCCNppp, 1 = CN\frac{1}{2}\frac{1}{2}$; in the 4-valued, $\frac{1}{3} = \Pi pCCNpCNppp$, $\frac{2}{3} = CN\frac{1}{3}\frac{1}{3}, 1 = CN\frac{1}{3}\frac{2}{3}$; in the 5-valued, $\frac{1}{4} = \Pi pCCNpCNpCNppp$, $\frac{1}{2} = CN\frac{1}{4}\frac{1}{4}, \frac{3}{4} = CN\frac{1}{4}\frac{1}{2}, 1 = CN\frac{1}{4}\frac{3}{4}$; and so on. The two long axioms in the 3-valued C-Π postulate-set on page 59 may be shortened in this way to $C\frac{1}{2}N\frac{1}{2}$ and $CN\frac{1}{2}\frac{1}{2}$; in which form they may be seen to presage Słupecki's 1936 completion of Wajsberg's 3-valued C-N postulates with $CTpNTp$ and $CNTpTp$, where $Tp = \frac{1}{2}$ whatever p is.

Dr. Woodger's translation might be criticized here and there, but he has done a difficult job well, and no more important logic-book has appeared in English in this decade.

<div align="right">A. N. PRIOR</div>

Canterbury University College,
New Zealand

The Delegates of The Clarendon Press have now agreed to undertake a collected edition of the writings of John Locke. The edition will begin with his correspondence, edited by Dr. E. S. de Beer.

It is probable that many of Locke's letters (or letters to him) are extant in private hands, either as part of family heritages or in formed collections of historical documents, and autographs. The Delegates would be grateful for any information which would assist the editor in making the edition as comprehensive as possible.

BACK NUMBERS OF 'MIND'

A member of the Mind Association has six bound volumes of *Mind* (1951 to 1956 inclusive) for disposal. Enquiries should be addressed to—

<div align="center">Mr. G. P. RENTON,
2 Capell Avenue,
Chorleywood,
Herts.,
England.</div>

X.—NEW BOOKS

Readings in the Philosophy of Science. Edited by HERBERT FEIGL and
  MAY BRODBECK, New York : Appleton-Century-Crofts, 1953. Pp.
  811. $6.00.

THIS large volume is made up of a collection of articles and some book
selections which have been published, with a few exceptions, during the
last fifteen years or so. The domain of the philosophy of science is as yet
not clearly defined. These *Readings* are therefore to be welcomed in
helping one to see at least what sort of problems are being discussed under
this heading,—with the mental proviso that the choice of selections by the
editors must inevitably force our interests into one direction rather than
another of what clearly is a very wide field. May Brodbeck, in an intro-
ductory essay, after dismissing some alternatives such as a socio-psycho-
logical study of science, or again, " Philosophy of Nature ", maintains
that the task of the philosopher of science is the " logical analysis of scien-
tific concepts, laws and theories " (p. 5) ; and H. Reichenbach writes
that such a philosopher " is not much interested in the thought processes
which lead to scientific discoveries ; he looks for a logical analysis of the
completed theory . . . " (p. 197) ; This approach was influenced by the
writings of Kant, Mill, Helmholtz, Poincaré and Mach ; in addition its
methods and problems have been profoundly affected by the new de-
velopments in logic and mathematics. On the side of pure science,
Relativity and Quantum theories have contributed to pose new problems,
such as the relationship of geometry to physics, or those of causation and
probability. Certainly the selections give the impression that these fields
have held the centre of interest of our most prominent " philosophers of
science ". Biology, Psychology, Sociology, Economics and History play
only second fiddle, often taking their cue from the formulations of the
methods of physics.

The volume is divided into nine main sections, which include the follow-
ing : (I) The Nature of Scientific Method ; (II) Philosophy of the Formal
Sciences ; (III) Space, Time and Relativity ; (IV) The Logic of Scientific
Explanation and Theory Construction ; (V) Causality, Determinism,
Indeterminism, and Probability ; (VI) Philosophical Problems of Biology
and Psychology ; (VII) Philosophy of the Social Sciences. Among the
contributors to the forty-seven odd articles making up these sections, the
names of Feigl, Carnap, Reichenbach, Nagel and Bergmann are prominent
not only by supplying several contributions each, but also by setting the
tone of the enterprise, so to speak, which revolves round discussions
familiar in the writings of " logical empiricists " in the 'forties. In this
way the question of the " meaning " of scientific concepts receives a large
share of discussion in about nine or ten articles, beginning with a selection
from Bridgman's " The Logic of Modern Physics ". A second major
theme is that of " Explanation " both in the natural and the social sciences.
Causality is a third topic which receives extensive discussion. These are
all themes of " pure philosophy " though here mostly set within certain
scientific contexts. In a book such as this there must of necessity be a
great deal of overlapping and unevenness in standards, quality and

approach but since these writings (with few exceptions) have appeared in print before, there fortunately is no need for individual notice, and a few remarks will suffice. A large share of the book is taken up, in one way or another, with the question of the logical status of the explanatory propositions of a theory, though no unitary point of view emerges. The most important contribution is still that of N. R. Campbell, " The Structure of Theories ", which reads as fresh as when it was first published thirty-five years ago. Not all the propositions of a theory are directly verifiable ; in particular it will contain quantities that are not directly measurable, though a combination of them may well be. In other words, some sophisticated theories have an explanatory function just because they have unverifiable propositions which contain unobservable quantities. But what makes such a theory significant? It exhibits an important analogy. Such analogies are not just aids to the formulation of hypotheses ; " they are an utterly essential part of theories " (pp. 297 f.). This view is directly contradicted by Carnap, who maintains that an analogy " has no more than an aesthetic or didactic or at best a heuristic value ". We " understand " a theory if we can use if for the " description of known facts or the prediction of new facts " (p. 317). Hempel and Oppenheim, in their very important article on " The Logic of Explanation ", maintain that the basic propositions of a theory (the " explanans ") must be true to make the theory an acceptable one (p. 322), whereas W. Kneale argues that such propositions cannot in principle be " tested directly in any way " (p. 353), that their whole value lies in their power to lead deductively to verifiable conclusions. Against all these, some of the social scientists seem to have required that theory-propositions must not only be true but self-evident (e.g. J. W. M. Watkins' quotations from J. M. Keynes, pp. 735-736) so that they may possess genuine explanatory power, give us genuine " understanding ". This leads to articles analysing the " Operation called Verstehen " (emphatic understanding), and the concept of " Ideal Type " in the theories of Max Weber. In all this, the selecting hands of the editors of course make themselves felt. When a notion such as " empathy " is discussed only by critics, the most sympathetic hearing does not always emerge. One might have wished sometimes for an account of " the other side " ; here, for instance, Collingwood's important arguments as presented in his " Idea of History ". Instead of this we get one solitary reference to this author (in a footnote on p. 740) and his name is not mentioned in the index. Similar remarks apply to such unfashionable concepts as Dialectics, Teleology, Vitalism, let alone the bête noire, Metaphysics. Yet one sometimes feels uneasy. With all this anti-metaphysical bravado, here and there difficulties seem to crop up, which a cursory reader might miss but which ought to involve some genuine philosophical difficulties and which are just the kind that led (or misled) the older philosophers into "metaphysics" Thus Hempel and Oppenheim find themselves unable to arrive at a satisfactory analysis of " lawlike sentences " (pp. 341-342) because of the difficulties surrounding " causal universals " and the problem of counterfactual conditions (p. 339). The authors refer to R. M. Chisholm's original article on this problem in MIND, for 1946. But did not this article arrive at the conclusion that the analysis of lawlike statements appears to require a reference to a " connexion " which is an " irreducible ontological category " ? (MIND, lv, 306). And are such requirements not typically the stuff of which metaphysics is made ? In a section specially contributed to " Notes on Causality ", Herbert Feigl tells us that the " concept of causation " has now been purified bv " the elimination of . . . metaphysical, i.e. in

principle unconfirmable, connotations . . ." (p. 408), being now " defined in terms of predictability according to a law ". A few pages later, the author also strikes trouble with the concept of law. For a proper analysis we require " conceptual tools that are not contained explicitly in extensional logic or in Hume's somewhat too psychologistic empiricism ". Here a doubt suggests itself which is not stilled by the brave assertion that " nevertheless, the basic tenets of a logical empiricism are thereby not called into question " ; " only the logical structure and epistemic role of natural laws may require fresh analysis " (p. 416). Why " only " ? When Wittgenstein, in the " Tractatus ", analysed extensionally the " truth " of a molecular proposition into a matrix of pairs of possible truth-values of atomic propositions, he thought that in so doing he was showing something about the world. It wouldn't be any good simply to supply an alternative analysis *defining* a certain implication relation as " basic " or *sui generis* ; it has to be shown that such an attempt has some more fundamental significance, for what (otherwise) is the point of such an analysis ?

This leads to some more general criticisms. Who is the reader most likely to benefit from this collection ? A professional scientist will have some difficulty to discover much relevance to his day-to-day work, and even a philosophically inclined one may be impatient with the formalisation of certain theoretical arguments which he cannot off-hand discover to contribute much to a greater understanding of the issues involved. The answer to this might be that such analyses are intended to bring out the logical structure of the arguments involved. But a doubt lingers whether such formalisations are not doing violence to the spirit of science as an *open* enquiry. The editors, in my opinion, seem to fall between two chairs. To appreciate the philosophical point of many of the discussions here presented, not enough philosophical material is put into the hands of those students who might be expected most to benefit from a reading of these pages. And on the other hand, the type of analysis which has most benefited the progress of science, for instance, Ernst Mach's work on MECHANICS AND HEAT, or even the type of approach to be found in the books of Conant, are given rather too little attention. A second criticism appertains to the nature of any such *Readings*. When the individual article appears within the framework of a volume that may be put into the hands of an elementary student as an official text then its arguments may become invested with a somewhat misleading authority. Sometimes, if the student takes sufficient trouble, he may discover differences of view, such as those remarked on already, despite the superficial appearance of the existence of " *a* philosophy of empiricism ". To give another instance, Reichenbach (p. 96), says that " the interpretation of probability and induction is highly controversial to-day ", and later claims to have formulated a " theory that . . . makes possible a solution of the problem of induction " (p. 456). Bergmann, on the other hand, says that there is little place in philosophy of science for such discussions. In the light of what we have found in the writings of Feigl, Hempel and Oppenheim, we are perplexed to read that " to the scientist it is obvious that there is no point in ontological speculation about lawfulness " ! (p. 275). And yet a third writer, A. Pap, holds that it is arbitrary whether one holds inductive generalisations to be testable scientific propositions or " generalities which prove fascinating only to philosophers ", and that, anyway, the dispute is only a " terminological quarrel ". I think that only a very highly trained student will fail to emerge utterly bewildered from the reading of such passages. A special difficulty is, of course, the unevenness of

standards. There are little masterpieces of introductory presentation, *e.g.* Cohen and Nagel's " The Nature of a Logical or Mathematical System ", side by side with the complex writing of Carnap's "Testability and Meaning ", or Bergmann's " Logic of Quanta ", the latter presupposing specialised knowledge of quantum mechanics not elsewhere expected.

Nevertheless, for those who care to find them, there are occasions when the selection of articles is such as to give us some useful contrasts.   One of the most successful are the two selections from Poincaré and Schlick on the problem of the " conventional " nature of the major laws of physics, Poincaré arguing for and Schlick against conventionalism.   But even here it may be disturbing to a tyro who has just read these two discussions, to find a later contributor *dogmatically* asserting that " the laws of motion are not laws in any ordinary sense but rather postulates which define the idea of force . . ." (Kneale, p. 359) ; still later to be followed by P. E. Meehl, in his closely reasoned contribution on " Law and Convention in Psychology ", who holds that there isn't really anything conventional about this because the " convention " doesn't tell you, let alone predict, just what sorts of forces there do and will exist (p. 653).   And so we could go on.   Next to highly sophisticated views on physical theorising by such writers as Duhem and Campbell, we get the rather more primitive pronouncements on the nature of scientific method, according to which we initially collect observations into laws, and only *then* proceed to the construction of the relevant theories (*Cf. e.g.* Zilsel, p. 715.)   Finally between them, some of the writers still adhere without further ado to a number of views which recent discussions have at least shown to be arguable.   Among these are Hempel and Oppenheim's assumption that one can speak of the " truth " of a theory or a law without qualification (pp. 322, 337) ; Beck's assertion that Millikan's apparatus and electrons are " constructs " *in the same sense* (p. 372) ; or Carnap's dictum that both universal and particular sentences " can never be completely verified " (p. 48).   The danger of a subject such as Philosophy of Science is that we may be in such a hurry to " do science " that we forget the " philosophy ".

Many of the articles have their own bibliography, and there is in addition a selected bibliography at the end.   Altogether, this book is obviously a most worthwhile addition to the library of anyone interested in the Philosophy of Science.   It would be impossible in any other way to obtain in such a convenient form a bird's eye view of one of the possible approaches towards this subject.

<div align="right">G. Buchdahl</div>

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The Value Judgment. By W. D. Lamont.   Edinburgh University Press (No. 5, History, Economics, Philosophy Series), 1955. Pp. xv + 335. 25s.

By ' the value judgment ' Dr. Lamont means " the assertion that something is good or bad, better or worse than something else ".   In the main the book is not about assertions of this kind, but about what he calls " the mental activity in which we attribute goodness or value to things ".

He holds that value judgments must be distinguished from ' efficiency ' judgments, aesthetic judgments, and moral judgments.   Moral judgments involve concepts of a juridical kind, whereas value concepts are of an

economic kind. In his previous book, *The Principles of Moral Judgment*, the author discussed legal ideas for the sake of the light they throw on moral ones. He now examines economic concepts as a means to working out a theory of the value judgment. He argues that while the economist is concerned with social phenomena, he must, for the purpose of explaining these, make certain basic assumptions about the operations of individual minds. These postulates are about the process of valuation in the individual mind, and to question them is to question the whole of economic theory. When it is thought, *A* is better than *B*, it is never two existent objects which are evaluated, but two states of affairs which do not yet exist, for such valuation is necessary only when choice is enforced, and choice is always of a future state of affairs. Both *A* and *B* must be 'contents of demand' in this sense, and *A* will be judged better than *B* if the satisfaction in having *A* without *B* is greater than the satisfaction in having *B* without *A*. Such satisfactions must be commensurable; they must therefore be the result of satisfying particular and diverse constituents of a single 'common demand', and the satisfaction of this demand must be expressible in quantitative terms of more and less. Contents of demand which are not objects of a common demand may exist in one consciousness, but as soon as a choice is forced between them, a new common demand which includes them both must come into existence if a rationally based choice is to be made. The problem: what forces us to generate a new common demand in such cases?—is the same as the problem: what is the ultimate ground of all comparative evaluation? There is a psychical principle which impels us to make the best possible disposition of our resources in order to achieve the greatest possible good for ourselves. This in a situation of enforced choice implies that we must first decide what on the whole we most want, thus generating a common demand. Common demand is then implied in all comparative evaluation, but when necessary it is generated by this psychical principle, the principle of 'economy', which is the ultimate ground of the comparative value judgment. This process of evaluation in the individual mind is presupposed in economic theories about the behaviour of men in economic relationships. The concept of common demand is essential and cannot be replaced by the law of diminishing utility, since this law, properly stated, itself presupposes the concept of common demand. Moreover, the five important concepts concerned with supply and demand in economic theory—Joint Supply, Composite Supply, Joint and Composite Demand, and Derived Demand—are such that all except the first have implications which support the proposition that objects comparatively evaluated must be objects of a common demand.

The 'mental activity in which we attribute goodness to things' is, Lamont decides, the mental state called approval, which is primarily a conative state, though partly cognitive, in that it involves an awareness of the objective situation in which the judgment of approval is made. Such conative attitudes will often be teleological in the sense that they are determined by the conception of an end. But the ultimate conative attitude on which these teleological attitudes depend cannot itself be teleological, though we may be reflectively aware of its end; it will determine its own end. It must, however, be organic, in the sense that unless there were a system of attraction and repulsion, rather than a set of random reactions, there could be no approval and disapproval. They must be basic activity patterns of our nature, since they are not reflectively determined, and are the ultimate ground of the simple attribution of goodness.

Lamont ends with a discussion of the moral judgment and freedom in relation to his main thesis.

The first section of the book, on the comparative value judgment, is the more closely reasoned and effectively argued. There is much in it which will be of interest to the philosopher of economics, particularly the discussion of the ' economic man ', the nature of the ' economic relation ', and the laws of diminishing utility and returns. But the basis of Lamont's discussion of the relation between the ' theory of (comparative) evaluation ' and economic theory is open to question. It is not clear that the truth of the whole structure of economic theory rests on postulates about the operations of individual minds, nor even that the acceptability or applicability of this structure is so dependent. This is not to deny that such could be postulates of economics, it is rather to question their status. The economist is not likely to be dismayed by any criticism of his postulates so long as these serve as the bases of a system which works in practice. This may seem unbearably paradoxical : how can we be satisfied with a system based on ' false ' axioms, and how can such a system consistently result in accurate predictions ? The paradox results from regarding such principles as both at once statements about individual psychology and the axioms of a convenient formal system. As the latter, the question of their truth or acceptability is the same as the question whether the system is formally satisfactory, and whether it is consistently fruitful of accurate predictions.

The second part of the book is less satisfactory. A great deal of the argument proceeds by way of answering the objections of an ' objectivist ' opponent, for which, in the main, recourse is had to the work of Sir David Ross. But the writer's approach is so original that he has obviously been hard put to it to find material in past controversies which is directly relevant to his own points. Discussing the view that approval is cognitive, not conative, Lamont says that Ross's argument, that while " A is good " always expresses approval, a distinction must be made between what a statement expresses and what it means, either begs the question or contributes nothing to it. It is hard to see why this argument should be expected to be relevant. The writer tries to make it relevant, I think, by misrepresenting it ; he goes on to talk about whether cognitive, conative, etc., attitudes have a meaning as well as an expression. One would be inclined to think that he had simply not understood Ross were it not for the fact that he explains Ross's point quite clearly in an earlier paragraph on page 214. In the sense in which Ross is using the terms, it is nonsensical to talk of anything but statements, etc., as both meaning and expressing something.

The passages which the writer quotes from Ross's work in this connexion are mainly concerned with the nature of assertions and the use of the word ' good '. The argument loses cogency when it takes its departure from similar grounds. The writer bases a crucial argument on a comparison of " the language of approval and that of cognition " (pp. 232 ff.) and compares the verb ' to approve ' with the verb ' to cognize '. He says on page 228 : " When we speak of cognition, we are thinking of those aspects of psychical life, such as perceiving, reasoning, and believing, which are concerned with the apprehension of the nature of things ", and on page 233 : " ' I cognize x in A ' . . . could be contradicted, by necessary implication, by the statement ' A is not, in fact, x '." This is clearly wrong if cognizing includes believing, at least in statements primarily about the speaker's beliefs. Moreover, what he says of the language of

approval is open to objection either as an examination of the language of approval or as an examination of Ross's examination of the language of approval. The writer says on page 232 : " ' I approve A ' is equivalent to ' I apprehend A to be good ' ' " ; on page 236 : " ' I approve A ' is the same as to say ' A is good' " ; on page 251 : " ' I approve A ' is substantially equivalent to ' A is good ' ; and on page 236 he agrees with Ross that " to express approval of A is not to say ' I apprehend or cognize goodness in A '. This would be to say something about my state of mind. To express approval is to say ' A is good ' ". Not all these statements can be correct. He says also, on page 237 : " ' I approve A ' can be contradicted by ' You do not approve A ' or by ' A is not good ' ; and ' I do not approve A ' can be contradicted by ' You do approve A ' or by ' A is good '." Not even ' I approve of A ' and ' I do not approve of A ' are respectively contradicted by ' A is not good ' and ' A is good ', in the way that ' I know A is x ' is always contradicted by ' A is not x '.

It is often difficult to decide whether Lamont is making a point or stating a definition. Proposition IX, page 210, suggests that in this book ' goodness ' is a technical term. Earlier, he says that nothing can be better than anything else unless both things are good, and this is a necessary condition for making a value judgment. This is only explicable if ' good ' and ' better ' are technical terms the use of which is being laid down, since of course in their ordinary use the terms ' good ' and ' better ' are related like ' short ' and ' shorter ', not like ' red ' and ' redder '. In this case, the references to the ordinary uses of the word ' good ' both by the author and in the quoted passages from Ross, are puzzling. The connexion between such facts and an arbitrarily defined mental activity remains dark. Indeed, the whole question of the relation between statements in ordinary language such as ' liquidation is better than communisation ' and the mental activity in which Lamont is interested must be raised. How far is the language of value judgments related to the process of value judging ? Need there be such a process for there to be a proper use of this language on any or every occasion ? How far is any discovery about such a process relevant to questions about the nature of such language ? How far are facts about this language clues to the nature of this process ? Part of Lamont's answer would appear to be that the nature of statements about the good and the better will be understood by an examination of the ' activity of valuation ', since it may be seen to be possible only under certain conditions : for example, that goodness cannot be, or that goodness must be, a non-natural quality. These are facts about the ' objective order '. This suggests that there are two important areas of investigation, the mental activity and the ' objective order ', and this suggestion is reinforced on page 3. The stone-wall appeal to the facts of ethical experience revealed either in extrospection or introspection (' I don't have that intuition ' ; ' I don't have that emotion ') is now rarely heard. But while Lamont's method is of course more sophisticated than this, and designed to lead us out of such impasses, one does feel that something of this exclusive dichotomy is involved in his ignoring more recent advances in the consideration of value judgments as assertions rather than activities, with a consequent narrowness of approach. This is not, of course, to show that the approach he adopts must always be unfruitful ; all one can say so far is that in this book, his detailed arguments are not sufficient to make out his case.

If the writer has failed to find a definitive solution to an important problem, this should not deter anyone from studying his book, which is

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full of the incidentally interesting and is never unoriginal The style is free and clear. except perhaps with regard to the use of some technical terms. The book is well produced and has a good index.

A. PHILLIPS GRIFFITHS

Sovereign Reason : and other Studies in the Philosophy of Science. By ERNEST NAGEL. The Free Press, Glencoe, Illinois, 1954. Pp. 315. $5.00.

THE time has come to protest. *Sovereign Reason* has all the external trappings of a book : hard covers, a table of contents, an index. Yet what is it? Not a book but a collection of essays and reviews, merely conjoined, with the barest of editorial attention. Even what the table of contents describes as a single study—say " Eddington's Philosophy of Science " (pp. 216-225)—may turn out to consist of a set of reviews, written some years apart and with no eye, one trusts, on future publication as a segment in a volume. " The question is too involved to be settled in a review ", Professor Nagel says of a point in Russell's epistemology (p. 192). Exactly so : a review is not the place for settling questions. But a book is.

Nagel himself seems to be a little uneasy. The book, he tells us, has as much unity as contemporary philosophies of science, as if its disjointedness were a reflection of its subject-matter. This, of course, is absurd. Contemporary philosophies of science are certainly diversified ; it does not follow that a book describing them must be a collection of fragments— fragments composed some as contributions to a collection of philosophical essays, some as articles or reviews in literary-political journals, some as critical notices in philosophical periodicals, quite devoid, then, of any unity in tone and texture, to say nothing of continuity in the exposition and development of a problem.

If I choose this particular occasion to protest against the growing habit of composing books by accumulation, I do so out of a sense of disappointment. It was reasonable to expect a book from Nagel : so many hints have been dropped, so many suggestions tendered and so few developed, in the reviews and articles which he is now content to collect. Of course, one can now see more clearly what points lie nearest to Nagel's heart ; repetition is no less characteristic of such a volume than sketchiness. Without detailed development, however, a " philosophy " is still at the level of an aspiration, pleasing or displeasing. The detail is what makes it a theory.

Nagel, so much is clear, aspires towards a " naturalism " for which " every quality and event is a genuine occurrence in some complex process or context " (p. 55), there being no total context and nothing which lies above or beyond complex processes. If contemporary philosophers of science reject such a naturalism, this is, Nagel feels, because the doctrine prevails that the objects of science are themselves non-natural, forming no part of the flux of everyday experience. Scientific theories are analysed abstractly, as if they were purely formal structures, without regard to the " tacit rules " for applying them in specific situations (p. 110). As a result a gulf yawns between the abstractions of science and the " world of experience " ; desperate empiricists like Russell (pp. 161-216) and Reichenbach (pp. 225-266) elaborate instructions for bridging it ; " malici-

ous philosophies of science " (pp. 17-36)—as represented in the writings
of Maritain and Gilson—argue from the existence of the gap to the futility
of science ; idealists like Eddington or Blanshard (pp. 266-296) reject
experience in the name of a higher rationality. Nagel attempts to show
that none of these expedients is successful. Russell's theory of construc-
tions rests upon an untenable theory of " simples ", which speaks of them
at one time as objects of acquaintance, which then cannot be identified
and at another as " limits of analysis ", which then cannot serve as the
starting-point for constructions : Maritain and Gilson say nothing clearly
but in so far as they can be said to have a philosophy of science at all rest
it on an indefensible contrast between " how " and " why " ; Reichenbach's
conception of " weight of evidence " is an unstable foundation for our
" belief in an external world " ; Eddington's epistemology is a tissue of
ambiguities ; Blanshard is no more successful than his predecessors in
showing that " rational " relations are always internal. That leaves only
Whitehead (pp. 150-161) and Dewey (pp. 101-150). Whitehead began
well, by disputing the " bifurcation of Nature ", but lapsed into an an-
thropomorphic metaphysics. Furthermore, his analysis of such "scientific
entities " as points is abstract, formal, whereas what is really needed is
an account of the actual *use* of points by working scientists. Dewey
then—with some praise also for Peirce (pp. 58-101)—emerges as victor,
his opponents having all been disqualified. The essays on Dewey, indeed,
are an excellent exposition of a philosopher who stands badly in need of
it. If one has qualms, it is because Nagel's Dewey is too reasonable, too
intelligible, too little of a Hegelian. But at least one can better under-
stand Dewey's fascination for able American minds.

Nagel's criticisms are lively and acute, but by the nature of the case
are usually too fragmentary and dogmatic—as a review can scarcely help
being—to carry any great conviction. An exception is the critique of
Reichenbach ; there Nagel writes as one who is conscious he can refer us
to his *Principles of the Theory of Probability*, to which this review serves
as a pendant. The difference in atmosphere is remarkable. One can see
at once in what circumstance a review may be genuinely suitable for re-
publication—as throwing fresh light upon a position which has elsewhere
been worked out in detail.

Dewey's influence is exhibited in the general character of Nagel's essays,
as well as in their contextualism. One is bound to be struck, as some-
thing not now familiar in British philosophy, by the political character of
these essays, Nagel's serious concern lest philosophy should perish beneath
a flood of " pontifical dogmatism ", oracular wisdom, and " condescending
absolutism " (p. 27), by his willingness to " take sides ", to describe his
work quite unselfconsciously in the language of philosophical labels, by
the ontological note which runs through his essays and reviews, the ambi-
tion they manifest to describe the general character of reality, although
not of Reality.

Yet Nagel is a formal logician, the class of men most reviled by Dewey.
How can formal logic, one naturally wants to know, be reconciled with
" contextualist " naturalism "? In his " Philosophy and the American
Temper " (pp. 50-58), Nagel remarks with some asperity that his fellow-
contextualists, recognizing that " a logic, no matter how subtle, provides
no warrant concerning matters of fact unless it is supported by controlled
observation ", display " an almost pathological fear " of formal analysis.
The objection is obvious : how can a formal logic be " supported " by
" controlled observation "? If one says, as contextualists ordinarily do,

that logic simply *consists in* the observation of scientific procedures, this difficulty vanishes, but try to speak of logic as at once " formal " and " supported by observation " and it is bound to arise.

Or take another point : in the course of his criticism of Russell's " constructions ", Nagel rejects the view, essential to the theory of " basic propositions ", that logical constants do not indicate. " In the sentence ' If I am hot, then I perspire ' ", he writes, " the logical connective ' if—then— ' does not stand between the *names* of two sentences but is used to connect the two ' facts ' *denoted* by the two sentences ' I am hot ' and ' I perspire ' ; and in this respect it functions like the two words ' I ' and ' perspire ', to convey information about matters of fact which are not linguistic or psychological " (p. 198). Questions crowd thick : How can a *word* " convey information "? In *what* respect has a connection between facts a function parallel to that of a word which forms part of a fact-denoting sentence? Presuming that words can convey information, what information do the words " if . . . then . . . " convey? We can ask, but shall receive no answer. The more sympathy we feel with Nagel's aspirations—and my own sympathy could scarcely be greater—the more irritating it is to be fobbed off with *obiter dicta*.

The very virtues of *Sovereign Reason*, indeed, increase our dissatisfaction with it. Everywhere one is conscious of a clear, acute and independent mind. But it nowhere extends itself, never sufficiently displays itself to make a substantial contribution to philosophical discussion. These scraps and shavings were admirably adapted to the contexts in which they originally appeared ; for that very reason they do not make a book. A good review is not a good article ; and a good article is not a good chapter.

JOHN PASSMORE

The Philosophical Movement in the Thirteenth Century. By FERNAND VAN STEENBERGHEN. Nelson, 1955. Pp. ix + 115. 15s.

PROFESSOR VAN STEENBERGHEN of the university of Louvain is well known to students of medieval philosophy for his work on Siger of Brabant. The volume under review represents a series of lectures delivered at The Queen's University, Belfast, under the auspices of the Department of Scholastic Philosophy. In it Van Steenberghen discusses the right interpretation of the currents of thought in the thirteenth century. The headings of the six chapters are as follows. The interpretation accepted about 1940 ; The organization of studies and its repercussions on the philosophical movement ; Eclectic Aristotelianism (1200-1250) ; Saint Bonaventure or Augustinian Aristotelianism ; Siger of Brabant or Radical Aristotelianism ; Concerning the condemnation of 1277.

According to Mandonnet, Albert the Great created ' Latin Aristotelianism ' about the year 1240, and Aquinas continued his work. Before this we can find in medieval thought a Platonic-Augustinian current which existed before the thirteenth century and which came into conflict with Aristotelianism towards 1270. Van Steenberghen, however, rejects the widely-spread theory that an Augustinian philosophical current existed before the thirteenth century and prevailed during the first part of the century. The organization of the schools was indeed inspired by St. Augustine, and the latter was the principal (other than the Scriptures, of course), though not the only source utilized by the theologians. But

" the ' Platonic-Augustinian ' current conjured up by Fr. Mandonnet simply did not exist as a philosophical current " (p. 39). This is not to say, of course, that Neoplatonic themes, harmonized with Christianity, were not utilized both before and during the thirteenth century. But they were utilized also by Albert the Great and Aquinas. Neoplatonism was always one of the sources of medieval thought. Before the thirteenth century, however, we find logical studies and fragmentary essays in philosophy, but no philosophical systems. We then find a new and Aristotelian current which, under the influence of the newly translated philosophical literature, came into being in the Faculty of Arts about 1200 and joined the tradition of logical studies which had been represented by, for example, Abelard. This Aristotelianism was indeed eclectic. But what de Wulf called " ancient scholasticism " or " pre-Thomistic scholasticism " was not, as Mandonnet believed, an Augustinian current.

Having disposed of Mandonnet, the author turns his attention to M. Gilson and others on St. Bonaventure. According to Gilson, Bonaventure and Aquinas possessed two different, though complementary conceptions of philosophy. Each philosophy embodies a different spirit, that of Bonaventure being Augustinian and Christocentric in inspiration, that of Aquinas being inspired by Aristotle. Aquinas emphasized the autonomy of philosophy ; Bonaventure minimized it. Aquinas regarded Aristotle as the supreme philosopher, though he developed the latter's philosophy very considerably ; Bonaventure regarded Aristotle with suspicion and even hostility. According to Van Steenberghen, however, Gilson exaggerated Bonaventure's hostility towards Aristotle. The philosophy which he used was fundamentally Aristotelian. True, it was by no means a pure Aristotelianism ; for it contained doctrines drawn from various sources, from Avicebron for instance. And these various elements were not synthesized in a developed and harmoniously knit-together system. But this does not alter the fact that Bonaventure's philosophy was fundamentally Aristotelian in character. What Gilson and others regard as the Augustinian spirit is simply the Christian spirit, which is present in Aquinas' thought as much as in Bonaventure's ; there is nothing specifically Augustinian about it. Further, it does not characterize philosophy as such, but rather theology or, more widely, the whole synthesis of Christian learning, including philosophy when considered as part of this synthesis and not simply by itself.

In his chapter on Siger of Brabant the author attacks the view, taken over from Renan by Mandonnet and others, that Siger represents ' Latin Averroism '. In reality Siger and those members of the Faculty of Arts at Paris who thought as he did represented radical (and from the theological point of view heterodox) Aristotelianism. True, certain doctrines advanced by Averroes were embraced by Siger. But if the latter accepted for a time the monopsychistic theory of Averroes, it was not at all because it was the theory of Averroes ; it was because he thought that Averroes had interpreted Aristotle correctly. Hence it is misleading to speak of ' Latin Averroism ' in this connexion.

At the end of his book Van Steenberghen briefly discusses Gilson's views about the relation of medieval philosophy to theology. After having once maintained that Albert the Great and Aquinas were the founders of modern ' rationalism ' Gilson then came to speak of specifically Christian philosophies. More recently he has advanced the view that the scholastic philosophies owed their value, not only to the influence of Christian

revelation, but also to the influence of scholastic theologies. The best theologians make the best philosophers. Van Steenberghen rejects this point of view in decided terms. It is true that the most notable philosophies of the thirteenth century were created by theologians. But this is simply due to the fact that the normal course was to pass after a few years from the Faculty of Arts to the theological Faculty. The theologians were thus older and more mature men, and it is not surprising that it was they who principally developed philosophy. But it by no means follows that they were good philosophers because they were good theologians. " In my opinion, good scholastic philosophers make good theologians ; and not, good theologians make good philosophers " (p. 114). Further, if the philosophies of the thirteenth century owed their value to theology, how can the Thomists seriously hope for a Thomist revival ? For it would not be possible to engage in discussion on equal terms with non-Thomist philosophers who did not accept scholastic theology.

With a good deal of what the author has to say I found myself in agreement. The term ' Latin Averroism ', as used of the thought of Siger of Brabant, seems to me to be misleading in as much as it suggests an attachment to Averroes as Averroes. As for St. Bonaventure, when consulting his works before writing about him in the second volume of my history of philosophy I received the impression that Gilson had given an exaggerated picture of his position. However, Gilson's great authority in matters relating to medieval philosophy made me think that I must be wrong. Subsequently Van Steenberghen has led me to think that my first impression was justified. Of course, Gilson's view of the matter makes Bonaventure much more interesting and ' exciting '. But these are not adequate criteria for writing history ; nor indeed for doing philosophy.

On the other hand, I am not yet convinced that we can profitably dispense with the traditional theory of an Augustinian current of thought within philosophy itself, represented by Bonaventure and kindred thinkers. I am inclined to think that there is an Augustinian spirit which is not reducible to the Christian spirit. After all, a fair number of modern philosophers, in Italy for example, are convinced that there is such a spirit. At the same time it is very difficult to define or clearly describe it. And this, I admit, is a point in the author's favour.

On the question whether good philosophers make good theologians or good theologians good philosophers, I prefer to make no comment. But it is indeed clear that the leading thirteenth-century writers, not excluding Bonaventure, made a systematic distinction between theology and philosophy. And the author does well to insist on this point as against those who slur over it. Yet many people, I imagine, would be disposed to agree with Gilson's statement that " the true scholastic philosophers will always be theologians ", though they might not mean by the statement precisely what Gilson meant.

Van Steenberghen has already advanced his main views elsewhere. But this clear summary, with its indication of the author's replies to criticism levelled against the earlier presentation of his ideas, will certainly be of use to students. Gilson's achievements in interpreting mediaeval philosophy are too great and too universally-recognized for his personal reputation to suffer from a little, to my mind well-directed, criticism. But there are some who look on him as a Delphic Oracle. They would do well to study the points of view presented here and elsewhere by Van Steenberghen.

FREDERICK C. COPLESTON, S.J.

Theory of Games as a Tool for the Moral Philosopher. By R. B. BRAITH-
WAITE. Cambridge University Press, 1955. Pp. 75. 6s.

THE mathematical theory of games, due to von Neumann, is a child of
the present century. It deals in abstract form with features that are
common to a great variety of competitive situations, using for their
analysis advanced mathematical methods. Its principles have also been
applied to the investigation of economic problems (*cf.* J. von Neumann
and O. Morgenstern, *Theory of Games and Economic Behavior*, Princeton,
1944) and to that of military contest.

Von Neumann's principle of decision in a competitive situation involving
two opponents with diametrically opposed interests (in technical language :
zero-sum two-person games) may be stated as follows : note, for all
possible choices at your disposal, the maximum loss that you could suffer
through the choice of your opponent ; make that choice which minimizes
this maximum loss (minimax principle). The central mathematical
problem is this : when you discover that your opponent has also chosen
in accordance with the minimax principle, will you then find that it would
have been better for you to make some other choice ? To answer this
question, mathematical analysis needs, of course, more precise definitions
for what we have called " choices ".

A. Wald has shown how consistent foundations of mathematical
statistics can be constructed by considering the statistician to be a player
against Nature. R. B. Braithwaite analysed this attitude in his *Scientific
Explanation* (Cambridge, 1953) ; in particular, he dealt with the con-
tention that the minimax principle cannot be justified unless one assumes
that Nature discriminates intentionally among various possibilities. He
concludes that such a premise is irrelevant to the minimax principle,
which he calls the " prudential policy ".

In his inaugural lecture, " Theory of Games as a Tool for the Moral Philo-
sopher " (Cambridge, 1955) Braithwaite continues to apply the theory to
problems of philosophy. Now he considers a case where the interests of
the opponents are not altogether contradictory, so that by co-operating
both can obtain more than by strict adherence to their respective prudential
policies. As Professor of Moral Philosophy, he is concerned with justice
and fair play, that is with concepts which are relevant wherever co-opera-
tion is possible or even advisable between individuals with differing—
though not necessarily contrasting or even comparable—aims in a social
community. The case of two musicians, anxious to play the piano and
the trumpet respectively, but not simultaneously if they can help it,
illustrates the point at issue. It is true that the arbitrator's, or con-
ciliator's advice on the appropriate extent of give and take derives from
a mathematical, not from a philosophical analysis. In fact, it " rests
upon the discovery that the logic of the general collaboration situation
is isomorphic with the geometry of a parabola ". However, the outcome
is plausible by many if not all standards and Braithwaite is well satisfied
with this. He points out that he has " made a small contribution towards
realizing Condorcet's dream " of illuminating the moral sciences by the
torch of algebra. In conclusion, he expresses his belief that, in the
fullness of time, " economic and political and other branches of moral
philosophy will bask in radiation from a source—theory of games of
strategy—whose prototype was kindled round the poker tables of Prince-
ton ".

S. VAJDA

The Logic of Moral Discourse. By PAUL EDWARDS. Glencoe, Ill.; *The Free Press*, 1955. Pp. 248. $4.00.

IN this remarkable book Paul Edwards discusses the language of moral disputes and judgments, and the language and theories of moralists—subjectivists, intuitionists, emotivists. . . . In the course of the argument a rewarding excursion is made into the logic of taste-judgments (Chapter V, " The Steak at Barney's is Rather Nice ") ; and numerous detailed, often bizarre, illustrations-from-life are developed with great sensitivity to the varied functions of moral expressions in ordinary language contexts and with undoubted moral seriousness, despite a cheeky, racy style. The positive theory which Edwards constructs is too close-knit and complex to be rehearsed *in extenso* within a review : the points which follow merely skim the top of what will require the most sustained discussion.

The strict emotivist in ethics chooses his language so as to dramatize the gulf between " fact " and " value ". To him moral claims can be neither objective nor subjective, for they describe nothing. They cannot be true or false any more than an ejaculation or a song can be true or false. But the drama needs to be tempered ; people often do instance facts which back up one moral judgment and weaken another. Disagreement in attitude may be rooted in disagreement in belief ; and here there *is* room for true and false. The emotivist, recognizing this, may still insist that the clarification of beliefs is a philosophically uninteresting preliminary to the *distinctively* moral performance—the final expression of attitude. Or, like Paul Edwards, he may reverse the emphasis while retaining much of the emotivist analysis.

Most moral utterances, Edwards holds, refer in a shorthand way to a set of features possessed by the object being appraised, and simultaneously express a pro- or con-attitude to it for having these features. Such utterances are objective and either true or false—for the object either has or has not these features. Stevenson argued that a dispute between A and B is " settled " when A succeeds in altering B's attitude into conformity with his own ; and (outrageously) that *any* statements which produce this result are *ipso facto* relevant to the dispute. But Edwards claims that moral disputes can often be settled in a way far closer to the way we settle disputes " as to the facts ". A proves his case only by showing that the referent of his judgment does possess the characteristics which form its descriptive meaning. Further description (of the implications of some of those features) may settle a range of disputes which the first procedure fails to settle. Only if A and B dig in their heels over incompatible " fundamental moral judgments " is a dispute unsettleable. For such would not be a quarrel over " features " but about the " stand " appropriate to the situation. With respect to fundamental judgments Edwards offers an " emotivist " analysis ; although even here the label " emotive " is perhaps more misleading than helpful, since Edwards stresses the difference between momentary surges of feeling and commitment to a moral policy. We may argue a very long time about the morality of euthanasia or of permitting communists to teach in universities, before lighting upon *any* disagreement regarding fundamentals : indeed, there may be none.

But Edwards admits a class of exceptions : disagreements over *non*-fundamental judgments may occasionally prove unsettlable. The author and a friend once found the company uncongenial at a camp dining-table. They moved to another, enjoyed their meal, but at the cost of offending

the first group. Edwards and his friend still disagree over the morality of their move, although they well know the (quite circumscribed) set of features which are morally relevant, and share the view that having congenial company is good, offending people bad. They simply assess differently the morality of the particular *combination* of features present. But if a fundamental judgment is one which cannot be supported by reasons, just indicates a stand, this sort of case surely is indistinguishable from such a judgment—made, not about a moral rule, but about a group of features taken as one complex characteristic. Further : " fundamental " disagreements over moral principles may be rare, but unsettlable disagreements like the dining-table dispute are, I suspect, even commoner than Edwards suggests in his discussion of them. But if they are common, the edge is taken off the contrast he wants to make between the *practical* fruitfulness of his theory in settling disputes by the " empirical justification " of judgments (p. 240), and the barrenness of intuitionism which " just knows ". He is quite right, however, in claiming that *on the whole* a disciple of his is likely to argue out a moral dispute more thoroughly and patiently than one whose theory tempts him to appeal prematurely to " conscience ".

In " Grading ", Professor J. O. Urmson denied that " there is more than one sense of the word ' good ' ". It would be absurd, if " good " were " a homonym with as many punning meanings as the situations it applied to ".[1] Edwards does not think this " polyguity " at all absurd. In " that was a good holiday ", " good " *means* (does not contextually imply) the criteria applicable to grading holidays : it also carries expressive meaning—the pro-attitude. One comment on this. Whether or not to count criteria-reference part of the meaning may be a decision-issue. Yet, one does sometimes want to say of a child that he knows perfectly well what " good " and " bad " mean, although he has learned only a very few criteria-sets relevant to particular appraisals. One also wants some way of distinguishing the logic of " good " from the different logic of an undoubtedly polyguous word like " natural ".

Readers of R. M. Hare's *The Language of Morals* may be shocked to find Edwards claiming that imperatives may be justified by appeal to facts ; that some " follow from " facts. To Hare, there can be no imperative conclusion without an imperative in the premisses. The conflict, however, does not seem to go deep : there need be no quarrel between them. For Hare counts " if you want to . . . " clauses as covertly imperative (" If you want a neat haircut, go to Smart's ") ; and Edwards is careful to deny that imperatives follow inductively or deductively from statements of fact.[2]

The volume is attractively produced, but misprints require attention on pages 44, 54, 60, 71, 79, 101, 117, 131 and 157.

RONALD W. HEPBURN

Physique et Métaphysique Kantiennes. By JULES VUILLEMIN. Presses Universitaires de France, 1955. Pp. 360. 1400 Francs.

THE stated general aim of this book is to discuss ' la philosophie kantienne de la connaissance en fonction de la théorie kantienne de la physique '.

[1] *Logic and Language.* ii. 178, 176.
[2] Edwards does not discuss in the body of the work writings on ethics more recent than 1950. It is to be hoped that he will sometime comment at length upon developments since then.

To this end attention is concentrated mainly on the 'Metaphysische Anfangsgründe der Naturwissenschaft' (M.A.N.), the pre-critical works on physics, and the Analytic of Principles. The book follows the arrangement of M.A.N. with successive sections on Phoronomy, Dynamics, Mechanics, and Phenomenology. This plan and the title suggest, quite correctly, that physics rather than metaphysics is the dominant partner in the enterprise ; that M.A.N. rather than the Critique of Pure Reason is the focus, in so far as they compete for attention ; and that the comment is rather of a mathematical than of a philosophical kind. Such a treatment would be defective only if it were thought to provide a complete account of Kant's theory of knowledge ; otherwise it would merely provide a wise limit to the scope of the enquiry. In fact the scope of the book seems to be limited to an informative and technical commentary on M.A.N. and the pre-critical works that relate to M.A.N. : to a background survey of the controversies in mechanics in which Descartes, Newton, Huyghens, Leibniz and Kant were involved ; and to an attempt to link the views of M.A.N. to those of the Critique of Pure Reason. Even if a limited task this also constitutes a formidable challenge, which Vuillemin meets bravely, but with varying degrees of success.

In the various sections on the background controversies Vuillemin writes well and with apparent authority, and the same is largely true of his discussions of the pre-critical works, and his claims about the (inevitable) inadequacies of Kant's mathematical equipment. He describes in generously mathematical detail the puzzles over such notions as ' quantity of motion ', ' conservation of matter ' ' action at a distance ', ' force ' and , work ', and outlines Kant's various views on these topics. He shows importantly the extent to which Kant improved on some previous solutions, and the extent to which he borrowed from them. He appraises Kant's major contribution in M.A.N. to problems of mechanics, and indicates the ways in which it was limited by Kant's ignorance of such things as dimensonal equations, statistical laws, field theory and thermodynamics. Vuillemin discusses also in a technical way Kant's views on geometry, algebra and arithmetic, and again shows how they might have derived from the limitations of contemporary mathematics. For example, some motives for Kant's preference for geometry over algebra and arithmetic are provided from his naturally incomplete knowledge of vector spaces, and algebraic treatment of transformations and invariants. All these points are made within the framework of the commentary on M.A.N. and help considerably to clarify its claims.

In trying to relate these points, and the commentary, to the Critique, Vuillemin seems rather less successful. Sometimes the mathematical bias of his account exaggerates the influence of Kant's mathematical and scientific views on his philosophy. For example, although it is illuminating to reveal the limitations placed on Kant's views on geometry by his ignorance of notions like ' vector space ', ' transformation ' and ' invariant ' (52-54) it is surely an exaggeration to conclude that ' Kant's hypothesis of a geometrical intuition consists in the assumption of a vector space, the base for which is formed by three arbitrary non-coplanar vectors '. It seems a similar sort of exaggeration to claim (144) that ' The principle of intensive quantity is no more than the introduction of the infinitesimal calculus into the critical structure '. Again, in discussing the scientific defects of Kant's dynamics (194) Vuillemin censures the ' mistaken general method of Kant's physics and metaphysics ' as though these defects were necessarily a reproach to Kant's philosophical claims. No doubt Kant's

physics and metaphysics were very strongly connected, but M.A.N. is a different sort of book from the Critique, for all that.

At other times the displayed relations between M.A.N. and the Critique stand in need of criticism, rather than confirmation that they are Kantian claims. For Kant often alleges the connexion between the structure of M.A.N. and the table of categories, which Vuillemin dutifully indicates. It is true that Vuillemin suggests (272) that Kant derived the categories from the principles, and the table of judgments from that of the categories. But even this leaves much to be explained, and especially those passages in Kant where the reverse order is relied on. The links between the various sections in M.A.N. and the principles are more obvious, though even here they often appear to be unwarrantably artificial. The most convincing relation for both Kant and Vuillemin is that between the Analogies and the three laws of Mechanics. Here the connexion is sufficiently clear to make the differences between the two passages more important than their overt similarities ; for this would help to show the general differences between M.A.N. and the Critique. In this context, although Vuil'emin's account is informative, and although he discusses the differences in the two sets of principles, he seems again rather to exaggerate the similarities between them. His discussion of this connexion between M.A.N. and the Critique is nevertheless well worth having.

G. BIRD

Realms of value : *A critique of human civilization.* By RALPH BARTON PERRY. Harvard University Press (London : Geoffrey Cumberlege), 1954. Pp. xiv + 497. 6(s.

PROFESSOR PERRY offers this book as the sequel hinted at in the last sentence of his *General Theory of Value* (1926). In the first 85 pages he sketches, with more brevity than lucidity, his main views, unchanged in substance, on the nature of " value ". His enterprise is then to exploit his general examination of " value " for " the rectification of frontiers and the establishment of order among its historically authentic realms " (vii). These realms " coincide largely with man's major institutions ", and Professor Perry's chapters traverse the fields of ethics, social science, political theory, jurisprudence, economics, science, aesthetics, history, education, metaphysics, and religion. Unity and order are to be brought into these fields " by adhering constantly to a fundamental definition of value " (vii). This definition is the familiar one, that anything has value when it is the object of an interest (p. 3).

The merits of the resulting treasure-house of remarks are not those of a collection of studies each of which justifies itself by fresh working of its own, however restricted, field. For as the book breaks into chapters, so do the chapters into paragraphs and the paragraphs into dicta. Too many sentences do less to advance any argument than to provide fillers for the *Reader's Digest*. So few precious phrases are left unturned that there simply is not time for sinking shafts below the surface.

The book's claim to be a book at all must therefore rest on the extent to which a worthwhile pattern emerges from the assemblage of familiar material. Take the three chapters on political and economic questions as a test case. The content of the chapters adds to such a pattern practically nothing that is not conveyed by the bare listing of the " realms of value ". The essay on democracy could be an expanded encyclopedia

article ; the guiding thread, the fundamental definition of value, is not mentioned in it. The more general chapter points out (p. 211) that if morality is conceived as the harmonization of interests, and the state as designed to serve the public interest, then political principles are in part applied morally. Such a point, however, is neither new nor dependent on a theory of value and it need take, and only took, a few paragraphs. The chapter on economics explicitly treats the relation between economic value and "value"; but this discussion, in which the artificial concept of "value" might have been confronted with some of the live notions the word has stood for, is devoted to showing merely that exchange value and price depend in some way on interests.

It does not seem that the other chapters contribute more than those to the desired unity and order, or even that they follow the guiding thread more closely. The survey of human civilization remains such as one can make in any good library. The excuse, which is certainly available, for attempting the book's enormous task is that we need to see life steadily and see it whole. But the straightforward inventory of an author's visual field makes it impossible to read his book steadily or to read it whole.

Many will nevertheless value the comments on many subjects of so judicious a writer. Their difficulties will be due to his manner, for the style throughout is Decorated Academic, both in detail and in that perfect balance of the *Times* leader : ' Neither a pure socialism nor a pure capitalism will suffice ; each must borrow from the other, and serve as a check on the other " (pp. 293-294). But they will be rewarded by a great deal of shrewdness and good sense. An exclusively negative review would not do justice to the extent to which one can understand, from this book also, the distinguished position the author occupies in his social and intellectual community.

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Received also—

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Annual Conference on Philosophy of Science. The second annual conference of the Philosophy of Science Group will be held at Wortley Hall, University of Nottingham, from Friday to Saturday, September 20-21, 1957. Details will be announced later.